PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCIII.

PART II.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCIII.

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PHILOSOPHICAL

TRANSACTIONS.

XII. *A Description of a Transit Circle, for determining the Place of celestial Objects as they pass the Meridian. By the Rev.* Francis Wollaston, *LL. B. and F. R. S.*

Read May 9, 1793.

An instrument which, in one observation, is capable of giving with precision both the right ascension and declination of celestial objects, has always appeared to me one of the desiderata in astronomy. Though I had often considered the various methods practiced for ascertaining each, and turned it in my mind how I could contrive to make one instrument answer both purposes; I never could satisfy myself in what way to effect the one, without destroying the accuracy of the other; till one evening, at a meeting of our Society in the beginning of 1787, Mr. Ramsden mentioned to me his idea of reading off the divisions of an instrument, by a microscope having a micrometer in the field of view, which, being detached from the limb, could examine with accuracy the

distance of the nearest division from a fixed point. It occurred to me immediately, that this was the thing I wanted: because a circle attached to the telescope of a transit instrument, and passing in review before such a microscope, or a pair of such microscopes, would answer the purpose. I did not then know, that a microscope of that kind had been applied by the late Duc de CHAULNES, to his dividing engine, for determining the divisions; described minutely by him, and published in 1768; a copy of which is in our library. Neither did I then know of the same idea having been the foundation of RŒMER'S method of reading off the divisions on his *Circulus meridionalis;* an account of which was published by HORREBOW, in the beginning of this century; where a reticule of ten squares was made, by trials of its distance from the limb of the instrument, to coincide with a division of ten minutes on that limb. With them I was not acquainted, till after my instrument was already in some forwardness. Whether Mr. RAMSDEN took the first hint from either of them, and improved upon it, I cannot say. He has brought it into use among us: I certainly derived it from him; and to him I acknowledge myself indebted for it.

This method of reading off has, indeed, been applied already with great success to different instruments; but I do not know that it has ever yet been adapted to the transit. Circles of various kinds have been constructed with wonderful accuracy, yet all have been formed with another view; and their turning freely in azimuth, seemed to render them less fit for the purpose which I wanted; *i. e.* a circle, firmly fixed, and turning truly in the plane of the meridian by means of a transverse axis; with all the adjustments of a

transit at the end of the axis itself (which appear to me essential to a due performance); and at the same time with the opposite readings, and all the adjustments of the circles now in use.

On this idea the following instrument was constructed: and since there are some particular contrivances in it, which are new, I take the liberty of laying a general description of it before this Society; not by way of setting forth the praises of any one instrument; but that this may be known in all its parts, with the advantages and disadvantages of each, as far as I have discovered any; and that such of them as are judged useful, (if the Committee shall think proper to honour this paper with a place in our Transactions) may be adopted by others.

My first design was, not to have given orders for one myself, but merely to communicate the thought to those who might improve upon it. Accordingly, I mentioned it first to Mr. RAMSDEN, in 1788: but the multiplicity of his engagements, and the fertility of his own imagination, (at that time particularly turned towards contriving to make instruments move freely in azimuth) rendered him disinclined to listen to a scheme for one on another plan. The same was the case with Mr. TROUGHTON. I mentioned it likewise to several of my acquaintance: but (perhaps the trouble and expense attending the construction of a first instrument, seldom so complete as a second or a third of the kind, might be the cause) no one was set about. After three years waiting, and becoming, in the mean time, more and more convinced of the advantages of such an instrument to astronomy; and Mr. CARY being recommended to me, as fully qualified for the

purpose; though I am growing too old to expect to make many more observations, I gave orders for one of a size and form which I thought most convenient to myself. Observers know best what it is they want; and an instrument-maker who will condescend to listen to them, is a treasure. In this, as well as other respects, it is but justice to Mr. CARY to say, that he has answered the character which was given of him. He has shewn himself, during the whole time, very diligent and attentive; comprehending readily my directions; giving freely his opinion, and his reasons for dissent, if he disapproved of what was proposed; yet being willing to follow mine, if I still continued in the same mind; improving upon some of my hints; and executing in a masterly way every part of it.

The drawing accompanying this paper (Tab. XV.) will shew the general form of the instrument; and need very little explanation.

The whole stands on three feet, adjustable by screws. The bottom plate (of 21¼ inches diameter) turns in azimuth; not on a long axis, but on a center; and rides on a bell-metal circle, truly turned, and to which the bottom plate itself is ground. In this way it moves very smooth by hand; but it is capable of being turned by a winch, with tooth and pinion. The intent of its turning thus, is merely for the convenience of reversing the instrument: for, though it might be used out of the meridian, and for azimuths; yet, since it is designed principally for meridian passages, when it is in its place the whole is clamped firmly to the bottom frame by four clamps, which confine it to the circle on which it rides: and this method of turning proves itself to be

steady, by the levels on the bottom plate never altering in the least upon screwing the clamps.

The four pillars, and their braces, explain themselves. They stand over the bell-metal circle; and the clamps are placed near the foot of each, for greater steadiness; since they carry the Y*s* for the pivots of the transit.

The construction of these Y*s* is peculiar: they hang, as it were, in gimmals, though of a very firm kind, a drawing of which (Tab. XVI. fig. 1) will best explain them. They have a horizontal motion, smooth and steady: the T, or frame, AB, which carries them, turning on a perpendicular axis of $2\frac{1}{2}$ inches, CD, ground to its socket; on the outside of the plate EF, which connects them with the pillars, and resting on that plate, to which the bottom of the frame itself is ground likewise. In this frame they have a vertical motion: the Y*s* themselves carrying a horizontal axis at AB, which, consisting of two frusta of cones on each side, in contrary directions, with a collar over them, guards against any shake whatsoever, while it admits of the Y adapting itself to the direction of the pivot. The idea of hanging them in this way, as well as that of turning the whole instrument in azimuth on a ground plate, was suggested by our late member Mr. JOHN SMEATON; to whom the world has, during many years, been indebted for repeated capital improvements in mechanics.

By thus hanging the Y*s*, the pivots have a bearing on them from end to end; instead of riding on a bell-metal *ridge*, as is the usual method where the Y*s* are fixed, and cannot set themselves in the direction of the axis. This seems to be a better bearing, and much less likely to wear the pivots.

Yet, to guard against any wear, a pair of cylindrical

springs, included in a tube, are applied through rings within the connecting plate above mentioned. These carry, each of them, a pair of rollers, on which a brass collar at each end of the axis of the telescope rides. The springs may be used or removed, at pleasure; and they can be strengthened or weakened, by means of a screw at the bottom of the tube, so as to take off from the pivots any part of the weight that may be judged best: and, since they are in a line with the axis, and are made capable of obeying it in every direction, there is no danger of their deranging its adjustments, while they render its motion exceedingly light and smooth indeed.

The adjustments of the *Y*s are both of them at the same end of the axis, opposite to the divided circle and the microscopes; because the smallest adjustment of that end of the axis between the microscopes, would have affected them so as to require an entire re-adjustment of them too. At the farther end, the axis is perforated, to admit light for illuminating the wires. And I find that (to my sight at least) it is much most agreeable to close the aperture with a pale green glass. The axis itself is 18 inches long, exclusive of the pivots, which are about $1\frac{1}{4}$ each.

The microscopes need no description. They are on the same principle as those described by our late member Major General ROY. (Phil. Trans. Vol. LXXX, p. 145.) Mine are 9 inches long; the object-end at 2 inches from the limb of the circle. They magnify 24 times. One revolution of the micrometer-screw is equal to one minute; and the head is divided to seconds.

The fixed or stationary wire in them, is at the first notch, or minute itself; and it is adjusted by means of a plumb-line,

which hangs from the top plate, and passes by the side of the axis; at about 8 degrees, or 1 inch $\frac{3}{4}$, from the centre. For this purpose there are dots made on the limb, at a suitable distance on each side of the zero, both above and below, whether the telescope be horizontal or perpendicular either way. These are viewed through two compound microscopes, (of $5\frac{1}{2}$ inches long, and their object-glass at 3 inches distance from the limb) carried by the same frames as the other microscopes.

The cursor, or moveable wire, in the micrometer-microscopes, is adjusted much in the same way as General Roy's; excepting that the micrometer head is made to turn stiffly on the neck of the screw, so as to allow of bringing the point of zero to front the eye, without the trouble of re-adjustment, if it happened to fall behind.

It may be asked, since I use a compound microscope for viewing the wire, why I choose a plain plumb-line close to the limb, in preference to one in the combined focus of the glasses? My reason is this; I use a compound microscope, because my eyes do never, with any comfort, adapt themselves to a single magnifier; and in this way I have more light, and can keep my eye at a greater distance. I approve much of Mr. Ramsden's ghost, as it is called, where it can be used with safety. But in this instrument, I thought I could not confide in it, as the microscope must be on a different support: whereas the looking at the dot itself, and the wire together, through a tube of above 5 inches, and at a distance of 10 or 12 from the limb, could admit of little or no parallax. I had intended making use of the original dots of the divisions for this purpose; but they are so minute, that the

smallest wire capable of supporting a plummet eclipses them entirely.

There is of course a level (and a very sensible one it is) for adjusting the axis. The circle was ordered to have ten radii; that when the telescope is horizontal, and pointing to a meridian mark, there might be a vacancy between the cones, above or below, for introducing a level. In the brace between the pillars, over the moveable Y, (at A, Tab. XV.) it may be observed, the bottom bar is omitted; in order to give the better room for passing the level, without inclining it, or running any hazard of striking it. From the lower bar of the opposite brace B, over the fixed Y, there stands out a forked piece of brass, to receive the leg of the level, and direct it to its place; as also for keeping it upright when the foot stands on the pivot, and just allowing a very little shake, so as not to cramp it. By this contrivance the level is easily handled, and reversed, without danger of disturbing it or the instrument.

The top plate, as may be seen in the drawing, has a large opening cut more than half way across it. The design of this is, to allow you to observe quite up to the zenith, and a little beyond it, clear of all obstruction whatsoever. And since the whole instrument is capable of being reversed, or turned half way round in azimuth; when you have occasion to observe the transit of stars, in that part of the heavens where they would be intercepted by the plate in one position, it is entirely out of the way in the other.

The circle itself is of full two feet diameter at the divisions; being $25\frac{1}{2}$ inches at the edge. The undivided circle, on the side of the telescope next to the open end of the axis,

serves for strength and uniformity; and to it is applied the clamp for elevation. That clamp is so made, as to allow the circle to run freely all round if you please; not bearing at all against it, but supporting itself, and yet being easily removeable. It has no command over the circle whatever, when handled with care, excepting in the altitude of the telescope, by an adjusting screw when the clamp is set: and, as that screw has a milled head at each end, it is as conveniently turned from the one as from the other side of the instrument, to bring the horizontal wire to bisect the object.

The telescope is of 2 inches aperture, and 33 focal length. The object-glass does not slide within the tube; but screws into the end of a piece of false tube, of 4 inches length, which slides on the outside of the principal tube, and is fixed in its place, by 3 screws and collars running in grooves, when its distance from the wires is adjusted. In this way, one has the whole aperture of the tube; and no greater length than is absolutely necessary for use; which, in such an instrument, appeared to me to be an advantage. In some respects I find it so: yet, the hazard of disturbing the collimation, by touching the outside of the tube, is an objection.

The wires are not in one cell; but in two distinct cells, with their faces towards each other. The perpendicular wires are 5, at 35 seconds of time distance in the equator; and are adjustable horizontally for collimation by a screw. The horizontal wires are 3, at about 15 minutes of a degree asunder; placed so as just not to touch, but to pass clear of the other wires; and they are adjustable in collimation by another screw peculiar to them. The two cells have each a power of

turning separately on the axis of vision; but, when once the two sets of wires are brought to be truly at right angles to each other, the cells can then be fixed together, and turned together, and finally settled in their place by screws and collars at the outside of the tube. These things, I believe, are new: I thought they might be improvements on the usual method; yet I find the adjustment of the horizontal wires in collimation, might be dispensed with.

My reason for having 3 horizontal wires, and at about that distance, was, that after having ascertained what the difference is, I might observe the lower limb of the sun or moon at the one, and the upper limb at the other of the extreme wires, without much altering the elevation of the telescope, and removing the centre of the object, or preceding and subsequent limbs of the sun or moon, far out of the centre of the field.

The divisions on the circle itself come now to be spoken to. They were done by hand; and have been executed with great care. The original divisions are by dots or points, at every ten minutes. Within, is another row, by strokes or cuts; laid off from the points to every ten minutes likewise. The dots are what we will regard first: the cuts afterwards.

As it always appears to me convenient, in actual observation, to contrive that every thing shall do itself, as far as I can, and to leave the mind as well as the body at perfect ease, and totally disengaged from calculation; I considered, that making both the microscopes talk the same language, read off the same way, with the guiding figure always to the same hand, and the dot to be observed to the same hand too, and the readings always positive, would conduce much to

one's ease, and thereby very greatly indeed to the accuracy and certainty of the observation.

With this intent, since the microscopes are, the one above, I ordered that to be marked A; the other below, B; considering that the numbers deduced from them could never be mistaken, if one got into the habit of examining A first, and noting that down, and then examining and setting B under it; which, if all things are true, ought to be the complement to 90 degrees.

To make the reading pleasant, I ordered the micrometer-screw in each to be placed on the right hand; and considered the moveable wire as always to be kept to the right hand of the other. This will of course, in all cases, measure the distance of the fixed wire from the nearest dot apparently on the right, (or, since the microscopes invert, the nearest dot really to the left) which will be either the degree itself on that hand, or some multiple of ten minutes from it.

That the numbering of the degrees might coincide with this idea, I considered, that the figures should be made to appear erect in the microscopes, in every position of the telescope (which they might be whenever it does not point below the horizon) and that they should be reckoned backwards. To effect this, they ought to be reckoned backwards in themselves, but to stand the contrary way, or inverted in reality. This would be different in the two microscopes, in respect of the centre of the circle; but that could create no difficulty. For, since the two quadrants nearest to the object-end of the telescope, would always be those coming under the examination of microscope A; and the two nearest to the eye-end, those to be observed at microscope B; they might be

figured accordingly. Hence, supposing the instrument placed in the meridian, with the graduated face turned towards the east; if, when the telescope is horizontal and points to the south, the upper quadrant nearest to the object-end, be numbered from that end from 1 to 90°, with the heads of the figures towards the centre of the instrument; and the other upper quadrant be numbered from the eye-end, with the feet of the figures towards the centre; they both would give the zenith distances of the objects observed. The former, at microscope A, while the telescope points to the south of the zenith; the latter at microscope B, when you are observing towards the north.

The two other, or lower quadrants, follow a similar rule, and serve to shew the altitudes, if both be numbered from the quadrature, instead of either end of the telescope; those leading towards the object-end, being placed with their heads, while those towards the eye-end, stand with their feet towards the centre of the circle.

To render this more intelligible, I will annex a drawing of the divisions, (Tab. XVI. fig. 2) numbered indeed only to every ten degrees; though the instrument itself has a figure at every degree, that one may always be in the field of view of the microscope. Hereby it may be seen, that all on one side of the telescope give zenith distances, while all on the other side give altitudes; and yet, that the figures in both the quadrants nearest to the object-end are placed with their heads towards the centre, and all towards the eye-end with their feet. This became necessary; and though it was a little perplexing at first to contrive, and see executed properly, it is found very convenient indeed in use.

The interior divisions, or cuts, are also numbered at every degree each way, from the eye-end to the object-end of the telescope, with the feet of the figures always towards the centre. The use of them is likewise very great; not for reading off the observations, but for setting the instrument. For, at a proper distance from the main pillars, there is a small pillar, carrying a compound microscope with a wire in its focus; which being adjustable, and once set to the latitude of the place, gives immediately the north polar distance of any object seen; or, by fixing the instrument according to the polar distance of an object sought, one is certain of its entering, at the proper time, the field of the telescope, near the centre wire. This pillar for the polar microscope, is removeable to the other side of the main pillars; which becomes necessary when the instrument is reversed.

This in general is the form, and these are the peculiarities in the construction of this instrument; which, being designed for meridian observations, or transits, I apprehend may best be named a TRANSIT CIRCLE.

In the progress of it, when the divisions came to be examined in their proper position, as to the truth of the opposite dots being exactly in the diameter of the circle, an error was discovered, which occasioned a great deal of trouble, and much loss of time. When the microscopes had been adjusted with care, after turning the circle one way, they continued true, and the same dots shewed themselves to be perfectly in the diameter, however often the circle were turned the same way round: but on one or more revolutions the contrary way, the same dots ceased to appear true. This, it was thought, could arise only from some deviation in the centre. And, since the

Ys hanging in gimmals was a new experiment, this error was supposed to take its rise from some *shake* in them. They were examined; and were altered in various ways. Fixed Ys were then made, of the usual form; others of a larger; others of a more acute angle. The difficulty was still thought to continue. Recourse was then had to Ys in gimmals again, which I was unwilling to give up; and friction-rollers were applied to take off some of the weight. Still this error did continue in a small degree: yet was that degree so small, as not to be discernible at the polar microscope; nor, as far as I could see, at those belonging to the plumb-line; and sometimes scarcely so at the others, to whose greater magnifying power it seemed to be owing that it was at all perceptible. The cause I then supposed to be, in a disposition in the pivots to gather up the side of the Ys towards which they were turned. Yet was that not the cause: for what little motion there was, I found afterwards to be in a contrary direction.

This led me into discovering, and at last rectifying the defect. The original idea of hanging the Ys in gimmals, as was said before, was derived from Mr. SMEATON; who kindly shewed to Mr. CARY those which he had made to a small transit instrument for his own use. His ought scarcely, in strictness, to be called Ys; for he had made a little hollow on each side where the pivots would touch, as a sort of bed to receive them, and make the angle less pinching. This, Mr. CARY had imitated: and, though I did not mean he should, he did the same to the second pair he made, after trying the other kinds. Since it was done, I let them so remain till I got the instrument home; for I really found all

trials so disturbed by the shaking of carriages, while it was at his house, that I could make no satisfactory examination there myself. When the instrument was in its place, I tried every experiment I could contrive to discover the cause of this error; whether it could be in the microscopes themselves; any shake in them, or in the pillars, or in the hanging of the Y*s*. Finding none of these to be in fault; and, upon trying the instrument at every 10 degrees all round, perceiving the axis thrown backward instead of forward upon turning either way, it occurred to me, that any grease or other particles would have it more in their power to produce that effect in a sort of pivot-hole, (which the hollowed sides really are) than between two fair flat surfaces. I thereupon took out the Y*s*, and had them formed to an exact right-angle, with the whole sides perfectly smooth, and flat, and well finished: and, since that has been done, I really can discover no difference which ever way the circle be turned; but think I may now say that deviation is quite removed.

Yet, I apprehend, it would have been of no consequence if it had continued, or been greater than it was. For, since the readings are as it were in a line above and below the centre, and both of them positive; any motion of the centre towards the right hand, would give the dots, both above and below, the appearance of being more to the left than they ought to be; and thence would give the measurement too small, and that in an equal degree in each; so that the sum of zenith distance given by one microscope, and of altitude by the other, would thereby be less than 90 degrees, by just double the error. And if the axis be moved towards the left, the contrary would be the result; the sum would exceed 90 de-

grees, by just double that quantity. Hence, the difference from 90 degrees, at the same time that it gives a mean between the two readings, would reduce the error or deviation of the axis to nothing.

The instrument here described, is of the size I thought would be most convenient for my own use: indeed it is full as large as I should recommend ever to be made in that moveable form. It stands on a cylinder of one solid stone, of $25\frac{1}{2}$ inches diameter, and 3 feet 6 inches long, bedded on a pier of brick, well bonded together, and rising from a good foundation, deep in the earth. The stone is clear of the floor all round, and is very steady indeed: the instrument rarely varies at all, in any respect. It is adjusted in the meridian to two marks, the one north, the other south; so that now they are truly placed, the collimation of the telescope is easily examined, without lifting the circle out of its *Y*s.

I may be supposed partial to an idea which I have long entertained; but I confess, I should very strongly recommend the having an instrument of this nature, though more perfect, in every observatory; I mean a transit instrument, on stone piers, with a suitable circle and microscopes; that, whenever you observe a meridian passage, you may, at the same time, measure the exact altitude, or zenith distance of every object seen. The being obliged, in the common way, to have recourse to two different instruments, occasions the zenith distances to be much less frequently observed, than it is to be wished they were. It is true the British catalogue was, for the most part, deduced from observations with a quadrant alone; and so was MAYER's. But, though labour and patient perseverance, may enable an observer to allow for

any deviations in the limb, a quadrant is at the best but an imperfect instrument for right ascensions.

I believe, in the best observatory, I should confine myself to a telescope of 45 inches, with a circle of about 9 feet 6 inches. Such a telescope would have great power; and the whole would not be encumbered with too great weight. If the telescope be of 5 feet, the circle must be of 4 feet 6 inches. But I certainly should dissuade the ever going beyond that; and I doubt whether the great additional weight of metal, and the disparities there will be in such a mass, would not counteract the advantage of a longer telescope. Beside, it may deserve consideration, that in a larger instrument some parts may be out of the extent of the observer's arms, which he could wish to reach. Our late friend, Mr. SMEATON, was against a circle of above 9 feet diameter. Between stone piers, there must be a double apparatus of microscopes, &c. to use when the instrument is reversed.

In conclusion, it may perhaps be proper to add, since some gentlemen may feel inclined to ask, How my instrument has performed? whether in actual observation it does what was expected from it? To this, I think, I now may fairly answer in the affirmative; that I do find it a very useful instrument, and the best adapted, of any that I know, to the perfecting of our catalogues. For some time, I will confess, I had my doubts. I received it in the beginning of the winter, when the cold and dark weather made all examination of it irksome. As a transit-instrument, I soon was satisfied with its performance, even in respect of the pole star itself. It is very steady; and rarely wants any re-adjustment at all. As a circle, I was not. The deviation of the axis, though both ends, as far as I could

judge, seemed always to deviate equally, perplexed me much; and destroyed all confidence. The collimation in altitude (whose error when constant, is unimportant) appeared variable; and seemed to give uncertain conclusions. Whether that took its rise from the object-glass, or the wires; from the hanging of the plumb-line, or the microscopes; was doubtful. All these things it took up much time to investigate. But I think I now may say, these difficulties are all surmounted. The error in the Ys, it has been said already, is cured. The object-glass I suspected might have some little *shake*, from its being attached to a false tube on the outside, and therefore liable to be touched, instead of being within the tube of the telescope itself. This was made more certainly steady. The wires I was sure did not move. Neither did the microscopes, after I had set them perfectly at ease; for I found that in the first placing, I had a little cramped one of them. The dots came then to be examined, by which the plumb-line is adjusted; or rather, I should say, by which the circle is brought into a position for adjusting the microscopes. Here a small error was discovered. It has been mentioned already, that there are four pair of dots for this purpose. Though laid off, I am satisfied, at first with the greatest care, and strictly true; the opening or enlarging of them afterwards, to make them just visible on each side of the plumb-line, had occasioned some very small differences in them, in respect of their adjoining divisions or dots on the limb. The adjusting therefore to a different pair of these dots, which I had done, would necessarily occasion a difference in the collimation. This being avoided, by using always the same; and other causes of error being removed; the collimation for altitude seems now

to be as steady as can well be desired. I had more of these dots; because, the instrument being new, I could not be certain in what position it might be found most convenient to adjust. I now do it always with the telescope pointing to the zenith; and in another instrument, I should recommend the having no more than two dots for that purpose.

Some small errors I do perceive, which I believe are to be ascribed to the great power of my microscopes, which are too strong for works of art. It was against the judgment of Mr. CARY that they magnified so much; and I believe he was in the right. Some errors are certainly to be laid to the charge of my own eyes, which do not define objects as they used to do. But in general, I may fairly say, my observations of the same star, seldom differ from each other above 5 seconds in altitude, and most commonly they are much within that limit.

In observing, I always study to be as much at my ease as possible: and therefore I always sit, and use a prismatic eye-glass. To avoid touching the instrument itself, or even the stone on which it stands, I have four upright poles from the floor to the roof, with cross braces on a level with the bottom plate of the instrument; against which I may lean, while I observe, or when I handle any part of the instrument. These I find to be of great comfort and use. Against two of the poles I hang a curtain occasionally, to keep off the sun, or to lessen the false light when I observe a star in the day.

The two exterior horizontal wires, mentioned above, I find very convenient. They are really $14' 48''{,}5$ of a great circle distant from the centre. By means of them, I can, without any hurry, observe the preceding limb of the sun at 9 wires;

then set the lower limb to the upper wire, and read that off; set the upper limb to the lower wire; aim ready to observe the second limb of the sun at the 3d, 4th, and 5th wires; and lastly, read off the upper limb after the observation is ended. In this way, one has the meridian passage through the middle of the field, or within 2′ of it: and the meridian altitude of both the limbs, while the sun's centre is on the meridian; for the little alteration in altitude is soon done, and can disturb nothing.

Indeed, upon the whole, this instrument itself is capable of doing a great deal of good work; and convinces me fully, that one between piers would be highly advantageous to astronomy. As a transit, mine is perfect, so far as that size permits: indeed it is in fact to all intents a transit-instrument. And for altitudes; since the readings are totally independent of the circle, though you have it in your power to re-examine your microscopes by the plumb-line between each observation, if you please; you find there is no occasion for it. In that respect, it has the advantage over a quadrant. No force is used in setting this instrument: the whole, from its form, is counterpoised in itself: there is no more probability of deranging it in altitude, than in azimuth: and therefore, all you have to do in actual observation beyond a common transit-instrument, is, to bisect the star as it passes, or as soon as ever it has passed the meridian wire, and read off the microscopes afterwards. Thus every observation is complete; by ascertaining the right ascension and altitude of every object at once, and with very little trouble; which must tend greatly to the improvement of our catalogues.

There is one additional advantage in an instrument of

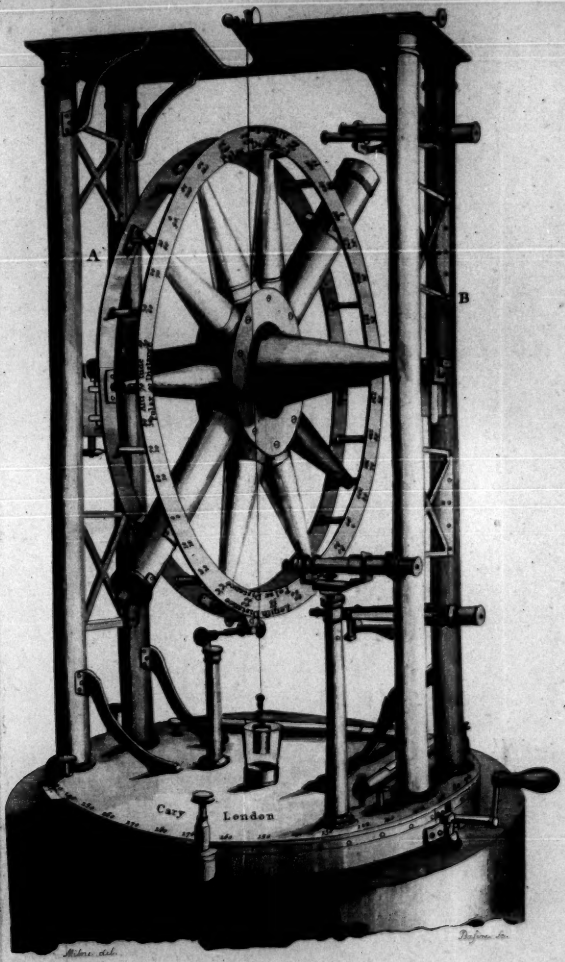

A
B
Cary
London

Fig. 1.

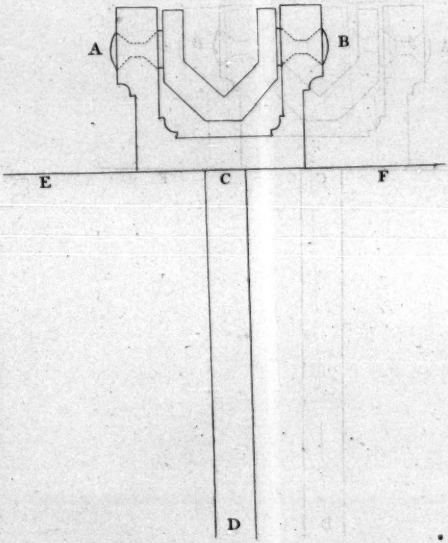
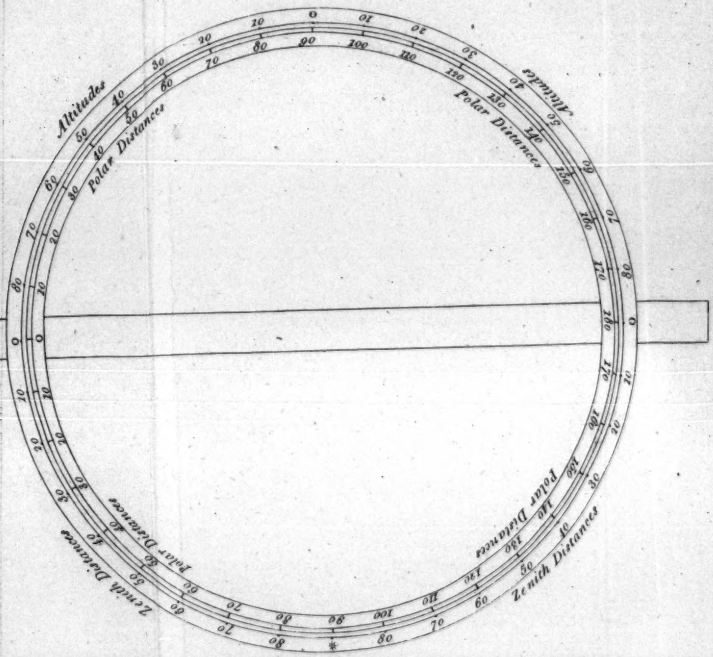


Fig. 2.


Altitudes
Polar Distances
Altitudes
Polar Distances
Zenith Distances
Polar Distances
Zenith Distances
Polar Distances

this form; that you have it in your power to reverse the whole in a few minutes without any hazard; which I do regularly; because thereby you discover, and destroy, any errors which there may be in the instrument itself, or which may at any time arise in observing.

XIII. *Description of an extraordinary Production of Human Generation, with Observations.* By John Clarke, *M. D. Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read May 16, 1793.

In the course of the last year, a woman was admitted into the General Lying-in Hospital, in Store Street, Tottenham-court Road, who, after a natural labor, was delivered of a healthy child.

The birth of this child was succeeded, however, by a repetition of uterine contractions, by which another substance was expelled, which is the subject of this paper.

It was inclosed in a distinct bag of membranes, composed of decidua, chorion, and amnios, and had a placenta belonging to it; the side of which was attached to the placenta of the perfect child. The membranes had been opened before I saw it, and a small quantity of liquor amnii having been discharged, the contents of the cavity were exposed.

The substance contained in the membranes was of an oval figure, rather flattened on the two sides. Its long diameter was about four inches; and its short diameter, from edge to edge, three inches. One edge was rather more concave than the other, and near the centre of it there was a small and thin funis, in length about an inch and half, by means of which it was connected to the placenta.

The surface of this substance was covered with the common integuments, and from it issued four projecting parts. Of these the upper is an imperfect resemblance of the foot of a child, having one large and three smaller toes upon it. The lower is a still more imperfect imitation of a foot, having one large and two smaller toes.

Between the two feet is situated a small and rounded projection; into which a small passage led, capable of containing a large bristle, but it soon terminated in a *cul de sac*. Close to the funis there was another small and thin projection, about a third of an inch in length, which looks like a finger, and was found to contain bony matter, and joints.

There was no appearance of head, or neck. No ribs could be felt, nor clavicle, nor scapula. There was no vestige of any thing like legs, or thighs, or upper extremities; or of organs of generation.

The only external similarity of this monster to a human fœtus, consists of its covering, and the attempt at a formation of two feet, and a finger.

Before the internal structure was examined, the navel-string of the perfect fœtus was injected, from whence the injection very readily passed through both placentæ, viz. that of itself, and that of the monster; and then into the substance of the monster also, as appeared by the redness of the skin.

When the injection had become cold, the skin was carefully dissected off; in doing which it was found that the upper foot had no bony connection, but grew loose, and only connected to the internal parts by cellular substance. The lower foot was articulated to the inferior part of the tibia and fibula.

The internal structure of the monster was composed of soft and bony matter. Upon cutting into the former, it appeared of a homogeneous fleshy texture, but without any regular or distinct arrangement of muscular fibres; and was very vascular throughout.

The bones which were surrounded by this fleshy substance were, the os innominatum, the os femoris, the tibia, and the fibula. The relative situation of these to each other described the attitude of kneeling. With regard to the bones themselves, the os innominatum, and the os femoris are both perfect, and of the size which we meet with in a fœtus at the full period of utero-gestation; but the tibia and fibula are much shorter than in their natural proportion to the thigh bone.

At the upper part, and towards the inside of the os innominatum, was placed a little portion of small intestines, loosely connected, by their mesentery, to the posterior edge of that bone, where it is commonly united to the os sacrum. These intestines had a covering of peritonæum, and were very minutely injected.

The next object was to trace the vessels of the funis, which was done with great care. There appeared to be only two, viz. an artery, and a vein; and these passed on towards the inner surface of the os innominatum. As they approached this bone, they gave off some branches to the surrounding parts, which quickly became too small to be traced. The trunks then passed backward, towards that part where the articulation with the os sacrum is generally found; at which place they went to the other side of the bone, where they distributed a great number of small branches, and were at length lost in the surrounding parts.

This was the whole of the internal construction of this very extraordinary monster. There was not the smallest appearance of head, or vertebræ, or ribs. There was neither brain, spinal marrow, nor nerves. It had no heart, nor lungs. It contained none of the viscera subservient to digestion, excepting the intestines already mentioned; nor any glandular substance whatsoever.

This being a monster of so singular a nature, I shall beg leave to add, to the foregoing description, a few observations, which the circumstances appear to me naturally to suggest.

The mere description of any monster is of very small utility, unless it tends to explain some actions of the animal economy, before imperfectly, or not at all understood. It is on this account that very little addition has been made to the stock of our knowledge of natural history, from considering those monsters in which there are either supernumerary or confused parts; because, if we cannot distinctly perceive the use, or necessity of parts, in their natural state, we are not likely to advance in information by the examination of those varieties of structure, where difficulties are only multiplied by the greater complication, or aggravated by the confusion of parts. The only useful inference in natural history, which can be drawn from monsters of the last kind is, that nature can deviate from the usual arrangement of parts, without any material inconvenience; and therefore, that the existence of parts so as to be capable of being applied to the purpose for which they are intended, in the perfect state of the system, rather than any precise order of them, is required for carrying on the functions of an animal body.

Monsters, however, where considerable parts are wanting,

seem peculiarly likely to assist in the prosecution of physiological researches.

If we were never to see an animal except in its perfect state, we could form no just idea of the comparative necessity of the different parts. So also, if we were to attend alone to the complete structure which obtains in the more perfect animals, we might be led falsely to conclude, that the usual connexion of parts, which we find in them, was essential to the structure and composition of animal matter. Of these parts, the brain and nerves, the stomach and digestive organs generally, the heart, and the lungs, would appear to be of such importance in the machine, that one would be induced to imagine that the functions of life could not be carried on without them: but in tracing the works of nature downwards, we shall at length find animals gradually becoming more and more simple in their construction. The brain and nervous system are altogether wanting in some, and there are others which have neither heart nor lungs; yet they continue to exist, and are capable of performing the most important functions of animals. Thus the formation of one animal serves to throw light upon the economy of others.

This great simplicity of structure is found, however, chiefly in animals the texture of whose bodies is nearly homogeneous; not consisting, as in more perfect animals, of parts so different from each other, as skin, intestines, &c. are from bone.

It might therefore still be supposed, that all the complicated mechanism, found in the more perfect animals, is essential to the construction of such heterogeneous substances, as those of which they consist.

To investigate this matter, we must have recourse to those monsters in which there is a deficiency of parts.

There is a very material difference between the nature of the life of the more perfect animals, during their time of fœtal existence, and after that they are born. In the latter state, the brain and nerves appear to be so essential, that any very considerable defect in them is incompatible with the well-being of the animal; but in the uterine state, considerable deviations from the ordinary arrangement of parts, and such as cannot be endured after birth, are supported without any inconvenience.

The brain has been frequently found very incompletely formed, and sometimes not at all, yet still there have been nerves. In other cases, where the brain has been perfect, the spinal marrow has been deficient in a great part of its extent, and sometimes throughout.

Both these occurrences are sufficient to prove, that, at any rate, that intimate connexion of the brain and nervous system, which takes place after birth, is not necessary for the formation of a body in other respects perfect. But still it would remain doubtful, whether any regular structure could be formed, without any vestige of either brain, or nerves; and therefore without a possibility of their influence, in any manner, toward such structure.

The monster now under consideration is so extremely simple, in this respect, that it cannot be exceeded by the most simple animal known.

It may be objected, however, that there might be brain, or nervous fibres, in this monster, but that they might, in the dissection, be destroyed. But, in the first place, I beg leave

to observe, that the parts were examined too carefully for such a suspicion; and, in the next, as there were no bones representing either the cranium, or spine, or os sacrum, it is not probable that their contents should exist in any other situation.

Another objection may perhaps be taken from the anastomosis of the vessels of the monster, with those of the perfect fœtus, and it may be assumed, that the nervous influence might be transmitted, in this way, along the vessels; but there is very good reason for believing that the vessels of the placenta have no nerves, since, when we cut the navel-string, neither the mother, nor the child, expresses the smallest sign of sensation: and indeed, even if they had nerves, it is still very unlikely that, merely by such anastomosis, any nervous influence could be conveyed.

I think it right to answer another possible objection which may be made, viz. that nervous matter may be co-extended, or co-existent with all other animal matter, and that, of course, it is of no consequence whether there be any sensorium, or reservoir of impressions, &c. or not; because the stimulus, which produces action, must reside in parts, as well as the other substance of which they are composed.

Now, although this may possibly be true, we have no evidence of the fact sufficiently satisfactory to carry conviction along with it. On the contrary, there seems to be good reason for entertaining an opinion, that nervous influence is conveyed from the brain downwards. If we are right in this conjecture, which is warranted by the experiment of tying, or cutting nerves, then the existence of the nervous fibre, like that of a string of a musical instrument, would be inactive,

unless it received an impression, which, with regard to the nerves, should come from the brain.

The whole of the actions of this monster, then, must have been those of the vascular system entirely; and these seem to have been capable of forming bone, skin, cellular substance, ligament, cartilage, intestines, &c.

The defect of heart (not an uncommon kind of monstrosity) proves, that the energy of the arteries was equal to carrying on the circulation, not only in its own body, but also through its own placenta.

The deficiency of nerves renders it extremely probable that their use is very small, if any, to the embryo.

It has been an opinion, entertained by a very acute physiologist, Mr. JOHN HUNTER, that, in all cases, a fœtus is a very simple animal, as to its internal actions, and the circumstances attending this monster fully confirm his idea.

The usual objects of nature in the formation of a fœtus are, that it should grow, and that it should be fitted with parts which, though of no use to it then, are essential afterwards. We know that the lungs are of this kind, and it is very likely that the brain and nerves are so too.*

The common uses of the nervous powers are, to convey impressions from without, and volition from within. Now a fœtus in the uterus is exposed to no external impressions, and is most probably incapable of volition, since it is not con-

* That there is a very material difference between the internal functions of a fœtus in the womb, and those of an infant after birth, seems very presumable; not only from finding that it can carry on life without parts which are of the greatest moment afterwards; but also from its possessing parts which after birth go into decay, or disappear, as the thymus gland, &c.

formable to the general wisdom of nature to give that which, in such a situation, must be useless.

The whole growth then, and formation of a fœtal body, would seem to depend upon the actions of the vascular apparatus, which, if we may be permitted to judge from this instance, is fully equal to the task.

With regard to the manner in which this monster was supplied with nourishment, and with the benefit of air, there is nothing remarkable; because it had a placenta, and the circulation between it and the mother was the same as in the most perfect fœtus.

Tab. XVII. and XVIII.

Exhibit the appearances described in the foregoing paper.

Tab. XVII. A view of the external appearances.

A. An imperfect formation of a foot, with four toes upon it.

B. An imperfect formation of another foot, having three toes upon it. This foot was connected to the tibia and fibula.

C. The projection into which a duct led, terminating in a blind pouch.

D. The funis umbilicalis.

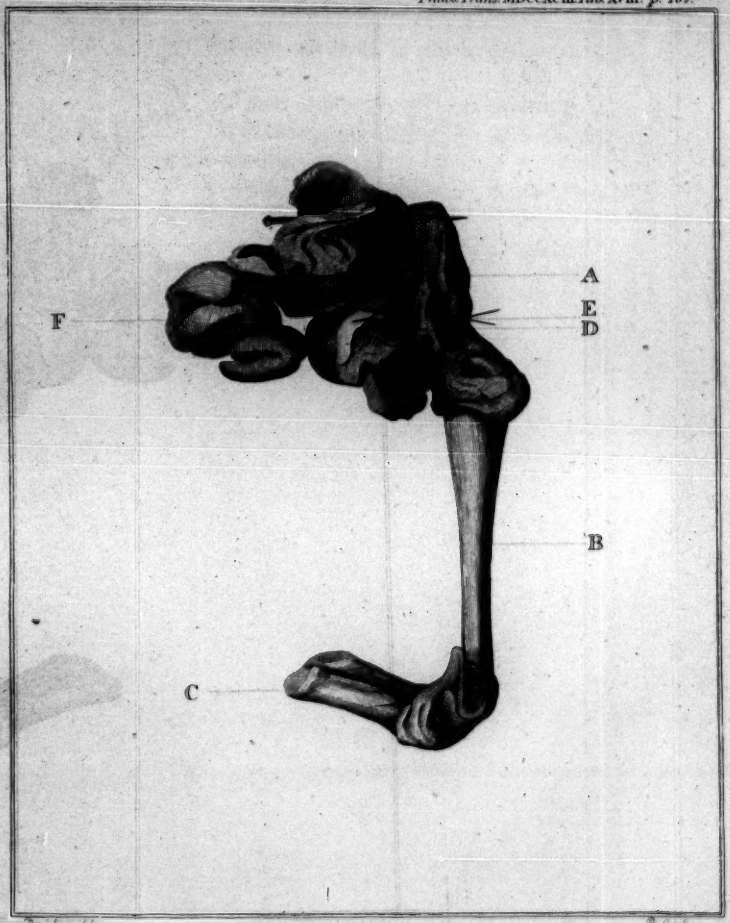
E. An imperfect formation of a finger.

Tab. XVIII. An internal view of the parts, as they appeared after clearing away the fleshy matter from the bones.

A. The os innominatum.

B. The os femoris.

A
E
D
C
B



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C. The tibia and fibula, to which the lower foot was connected.

D. The funis umbilicalis, with two bristles in the vessels.

E. The bristles passing, in the vessels, to the outside of the os innominatum.

F. A portion of small intestines, terminating in a *cul de sac* at each extremity.

XIV. *Description of an Instrument for ascertaining the specific Gravities of Fluids.* By John Godfrey Schmeisser. *Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read May 16, 1793.

Although it be well known to chemists, as well as to experimental philosophers in general, that the ascertaining the specific gravities of bodies is a matter of great importance in various chemical experiments, as well as in the analysis and chemical investigation of different substances; yet we find that this precaution is too frequently neglected in the accounts given of the experiments by the authors themselves, and that the neglect of it has sometimes occasioned the failure of these very experiments, when repeated by others.

As this defect has, in a great measure, arisen from the want of an accurate and convenient apparatus, a defect which I have formerly experienced myself, I have for some time past employed my thoughts in inventing a contrivance by which this difficulty might be removed. I flatter myself that I have now succeeded; having contrived an instrument, which I have found to answer every purpose for which it was intended, to my great satisfaction; so that the specific gravities of fluids may be determined in an easy and accurate manner.

Every ingenious man will easily convince himself in what respect this instrument may differ from, or how far it may be

preferable to, those which have been hitherto made public; and even to that lately invented and recommended by Mr. RAMSDEN.

The whole apparatus is represented in Tab. XIX. fig. 1; it consists of a flat-bottomed glass bottle (fig. 2) in which is fitted, by grinding, a glass stopper having a thermometer passing through it, (fig. 3.) The bore of this stopper is conical, (fig. 4) and the thermometer has a glass collar, (fig. 5) which is ground into the bore of the stopper, so as to be perfectly tight. There is some difficulty both in making the glass collar, and in fitting it into the stopper. If the thermometer tube and the collar be not made of the same metal, the collar is very apt to fly off in grinding; for this reason I have sometimes fixed the tube into the stopper by means of a thin piece of elastic gum, wound very tight round the tube. This gum, by its elasticity, effectually excludes air and liquids, and is, in the usual temperature of the atmosphere, not dissolved by any liquor, except vitriolic æther, and not even by that, unless it is particularly prepared for the purpose.

The cavity left at the upper part of the stopper may be filled up with sealing wax, or any other kind of cement; this will assist in fixing the tube, and as the liquors to be weighed do not come in contact with this part, if the bottle be carefully filled, there is no danger that the wax, or cement made use of, should in any degree affect the accuracy of the experiments.

I have made, at different times, comparative experiments with this instrument, with a view to the further ascertaining its accuracy, and the different improvements made in it; and I can with much confidence assert, that I have never

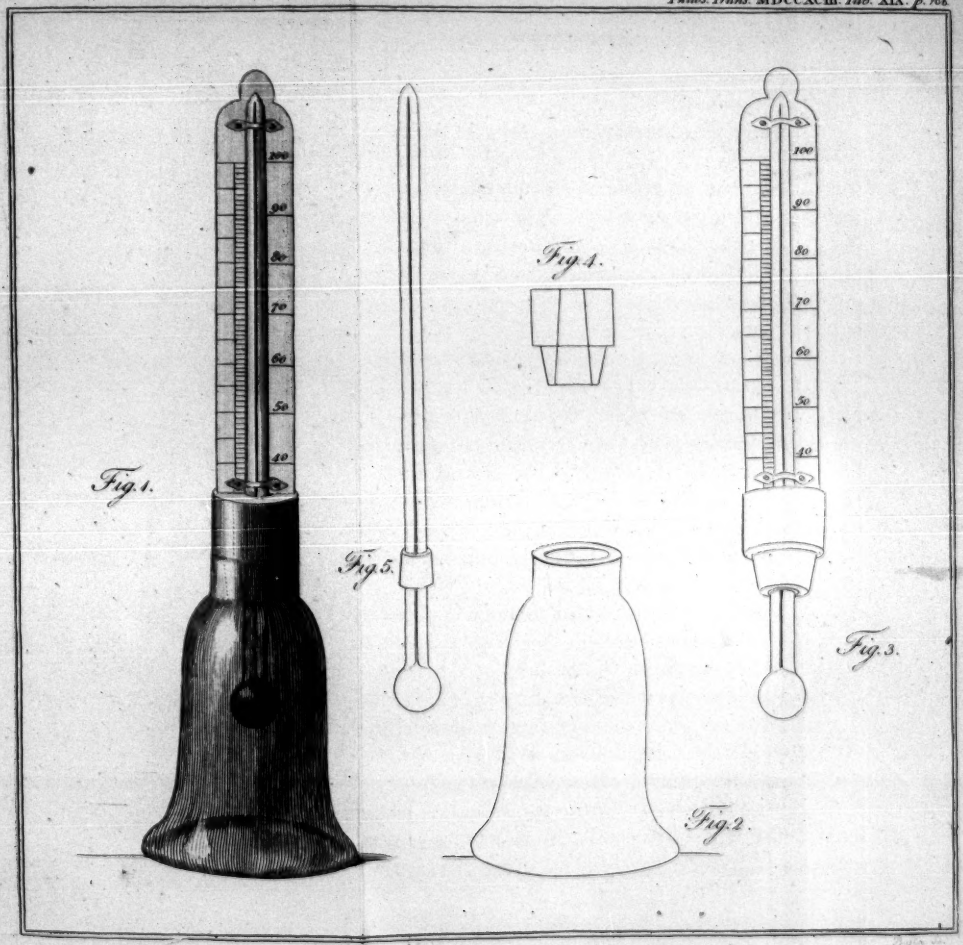
found either the least difference in the results, or any thing else contrary to my expectations.

The manner of using this instrument, and preparing it for experiments, is as follows.

(1.) A. An accurate cubic inch, which is fastened, by means of a horse-hair, to a hydrostatic balance, is to be suspended in a vessel with distilled water, of the temperature of 60 degrees, according to FAHRENHEIT; when the sum of the weight which the cubic inch thus loses, in the water, will be equal to the weight of an equal quantity of water displaced by it.

(2.) B. The instrument, free from moisture, is then to be put into the scale of an accurate balance, and its weight ascertained, from which the weight of the common air contained in the bottle must be deducted; when the remainder will indicate the absolute weight of the instrument.

(3.) C. The bottle of the apparatus is then to be filled with distilled water, of the temperature of 60 degrees, and the stopper, with the thermometer, fitted to the bottle, so that neither the smallest bubble of air may remain in it, nor any of the fluid adhere to the outside of the stopper or bottle; after which the weight of the water is to be ascertained, and marked upon the bottle, from which, by calculation according to experiment A, the quantity of water, contained in the bottle in cubic inches measure, may be found. Having thus ascertained the quantity of water of 60 degrees of temperature which the bottle contains, the bottle may then be filled with any other fluid of the same temperature, and its weight ascertained, according to experiment C, and compared with that of distilled water. If, for example, the bottle be found


100
90
80
70
60
50
40
Fig. 1.
Fig. 5.
Fig. 4.
Fig. 3.
Fig. 2.

to contain 327 grains of distilled water, and 654 of another fluid, the difference will be as 1 to 2; or 654 divided by 327, will give 2 for the quotient. The specific gravity then, of the fluid thus found, compared with that of distilled water, is properly expressed by the ratio 2,000 : 1,000; which latter expression is taken for the standard.

As it is a known fact that fluids exhibit different specific gravities at different temperatures, it would have been necessary for me to form a table, exhibiting the specific gravities of fluids at different temperatures, had I not, in order to avoid this inconvenience, hit upon a method of bringing the fluids, whose specific gravities are to be investigated, to a certain standard, viz. to 60 degrees, by setting the bottle with the fluid in a glass vessel with cold water, and adding as much warm water as may be necessary to bring that fluid to this standard of 60 degrees.

As the fluor acid will in some measure dissolve the glass, it becomes necessary, when that acid is to be weighed, to coat the inside of the bottle, by melting a little bees-wax in the bottle, and turning it, with the thermometer, in such a manner that the inside, together with the lower part of the thermometer, may become totally covered when cooled; which coating may easily be removed by means of a little oil of turpentine, or any other essential oil, all of which dissolve wax very readily.

XV. *Extract of a Letter from Sir* Charles Blagden, *Knt. Sec. R. S. to Sir* Joseph Banks, *Bart. P. R. S. giving some Account of the Tides at* Naples.

Read May 2, 1793.

Rome, March 30, 1793.

I took some pains at Naples to get information about the state of the tides, but could learn nothing satisfactory. The quantity of rise and fall is so little, that unless the sea be very calm, it is impossible to make a good observation. One of the best places for ascertaining the phænomena would be at what they call the river Styx, which is a narrow communication between the Porto di Miseno and the Mare Morto. Here I learned very distinctly that the water sometimes ran in, and sometimes out, but could not get the times; when I was there it was running out. The best observation I had was on the 2d of March, when it appeared to be high water at Naples about eleven in the forenoon, and low water between five and six in the afternoon; with a difference of pretty exactly one foot in the height. The wind blew the same way all the time, and the sea was very little agitated. On the preceding day the water had sunk an inch or two lower. From this observation, as well as some others less accurate, I concluded the time of high water at full and change to be between nine and ten o'clock, in the Bay of Naples.

I am, &c.

C. BLAGDEN.

XVI. *Observations on Vision.* *By* Thomas Young. *Communicated by* Richard Brocklesby, *M. D. F. R. S.*

Read May 30, 1793.

It is well known that the eye, when not acted upon by any exertion of the mind, conveys a distinct impression of those objects only which are situated at a certain distance from itself; that this distance is different in different persons, and that the eye can, by the volition of the mind, be accommodated to view other objects at a much less distance: but how this accommodation is effected, has long been a matter of dispute, and has not yet been satisfactorily explained. It is equally true, though not commonly observed, that no exertion of the mind can accommodate the eye to view objects at a distance greater than that of indolent vision, as may easily be experienced by any person to whom this distance of indolent vision is less than infinite.

The principal parts of the eye, and of its appertenances, have been described by various authors. Winslow is generally very accurate; but Albinus, in Musschenbroek's *Introductio*, has represented several particulars more correctly. I shall suppose their account complete, except where I mention or delineate the contrary.

The first theory that I find of the accommodation of the

eye is KEPLER's. He supposes the ciliary processes to contract the diameter of the eye, and lengthen its axis, by a muscular power. But the ciliary processes neither appear to contain any muscular fibres, nor have they any attachment by which they can be capable of performing this action.

DESCARTES imagined the same contraction and elongation to be effected by a muscularity of the crystalline, of which he supposed the ciliary processes to be the tendons. He did not attempt to demonstrate this muscularity, nor did he enough consider the connection with the ciliary processes. He says, that the lens in the mean time becomes more convex, but attributes very little to this circumstance.

DE LA HIRE maintains that the eye undergoes no change, except the contraction and dilatation of the pupil. He does not attempt to confirm this opinion by mathematical demonstration; he solely rests it on an experiment which has been shewn by Dr. SMITH to be fallacious. HALLER too has adopted this opinion, however inconsistent it seems with the known principles of optics, and with the slightest regard to hourly experience.

Dr. PEMBERTON supposes the crystalline to contain muscular fibres, by which one of its surfaces is flattened while the other is made more convex. But, besides that he has demonstrated no such fibres, Dr. JURIN has proved that a change like this is inadequate to the effect.

Dr. PORTERFIELD conceives that the ciliary processes draw forward the crystalline, and make the cornea more convex. The ciliary processes are, from their structure, attachment, and direction, utterly incapable of this action; and, by Dr.

JURIN'S calculations, there is not room for a sufficient motion of this kind, without a very visible increase in the length of the eye's axis: such an increase we cannot observe.

Dr. JURIN'S hypothesis is, that the uvea, at its attachment to the cornea, is muscular, and that the contraction of this ring makes the cornea more convex. He says, that the fibres of this muscle may as well escape our observation, as those of the muscle of the interior ring. But if such a muscle existed, it must, to overcome the resistance of the coats, be far stronger than that which is only destined to the uvea itself; and the uvea, at this part, exhibits nothing but radiated fibres, losing themselves, before the circle of adherence to the sclerotica, in a brownish granulated substance, not unlike in appearance to capsular ligament, common to the uvea and ciliary processes, but which may be traced separately from them both. Now at the interior ring of the uvea, the appearance is not absolutely inconsistent with an annular muscle. His theory of accommodation to distant objects is ingenious, but no such accommodation takes place.

MUSSCHENBROEK conjectures that the relaxation of his ciliary zone, which appears to be nothing but the capsule of the vitreous humour where it receives the impression of the ciliary processes, permits the coats of the eye to push forwards the crystalline and cornea. Such a voluntary relaxation is wholly without example in the animal economy, and were it to take place, the coats of the eye would not act as he imagines, nor could they so act unobserved. The contraction of the ciliary zone is equally inadequate and unnecessary.

Some have supposed the pressure of the external muscles, especially the two oblique muscles, to elongate the axis of the

eye. But their action would not be sufficiently regular, nor sufficiently strong; for a much greater pressure being made on the eye, than they can be supposed capable of effecting, no sensible difference is produced in the distinctness of vision.

Others say that the muscles shorten the axis: these have still less reason on their side.

Those who maintain that the ciliary processes flatten the crystalline, are ignorant of their structure, and of the effect required: these processes are yet more incapable of drawing back the crystalline, and such an action is equally inconsistent with observation.

Probably other suppositions may have been formed, liable to as strong objections as those opinions which I have enumerated.

From these considerations, and from the observation of Dr. PORTERFIELD, that those who have been couched have no longer the power of accommodating the eye to different distances, I had concluded that the rays of light, emitted by objects at a small distance, could only be brought to foci on the retina by a nearer approach of the crystalline to a spherical form; and I could imagine no other power capable of producing this change than a muscularity of a part, or the whole, of its capsule.

But in closely examining, with the naked eye in a strong light, the crystalline from an ox, turned out of its capsule, I discovered a structure which appears to remove all the difficulties with which this branch of optics has long been obscured. On viewing it with a magnifier, this structure became more evident.

The crystalline lens of the ox is an orbicular, convex,

transparent body, composed of a considerable number of similar coats, of which the exterior closely adhere to the interior. Each of these coats consists of six muscles, intermixed with a gelatinous substance, and attached to six membranous tendons. Three of the tendons are anterior, three posterior; their length is about two thirds of the semi-diameter of the coat; their arrangement is that of three equal and equidistant rays, meeting in the axis of the crystalline; one of the anterior is directed towards the outer angle of the eye, and one of the posterior towards the inner angle, so that the posterior are placed opposite to the middle of the interstices of the anterior; and planes passing through each of the six, and through the axis, would mark on either surface six regular equidistant rays. The muscular fibres arise from both sides of each tendon; they diverge till they reach the greatest circumference of the coat, and, having passed it, they again converge, till they are attached respectively to the sides of the nearest tendons of the opposite surface. The anterior or posterior portion of the six viewed together, exhibits the appearance of three penniformi-radiated muscles. The anterior tendons of all the coats are situated in the same planes, and the posterior ones in the continuations of these planes beyond the axis. Such an arrangement of fibres can be accounted for on no other supposition than that of muscularity. This mass is inclosed in a strong membranous capsule, to which it is loosely connected by minute vessels and nerves; and the connection is more observable near its greatest circumference. Between the mass and its capsule is found a considerable quantity of an aqueous fluid, the liquid of the crystalline.

I conceive, therefore, that when the will is exerted to view an object at a small distance, the influence of the mind is conveyed through the lenticular ganglion, formed from branches of the third and fifth pairs of nerves, by the filaments perforating the sclerotica, to the orbiculus ciliaris, which may be considered as an annular plexus of nerves and vessels; and thence by the ciliary processes to the muscle of the crystalline, which, by the contraction of its fibres, becomes more convex, and collects the diverging rays to a focus on the retina. The disposition of fibres in each coat is admirably adapted to produce this change; for, since the least surface that can contain a given bulk is that of a sphere, (SIMPSON's Fluxions, p. 486) the contraction of any surface must bring its contents nearer to a spherical form. The liquid of the crystalline seems to serve as a synovia in facilitating the motion, and to admit a sufficient change of the muscular part, with a smaller motion of the capsule.

It remains to be inquired, whether these fibres can produce an alteration in the form of the lens sufficiently great to account for the known effects.

In the ox's eye, the diameter of the crystalline is 700 thousandths of an inch, the axis of its anterior segment 225, of its posterior 350. In the atmosphere it collects parallel rays at the distance of 235 thousandths. From these data we find, by means of SMITH's Optics, Art. 366, and a quadratic, that its ratio of refraction is as 10000 to 6574. HAUKSBEE makes it only as 10000 to 6832,7, but we cannot depend on his experiment, since he says that the image of the candle which he viewed was enlarged and distorted; a circumstance that he does not explain, but which was evidently occasioned by the

greater density of the central parts. Supposing, with HAUKSBEE and others, the refraction of the aqueous and vitreous humours equal to that of water, viz. as 10000 to 7465, the ratio of refraction of the crystalline in the eye will be as 10000 to 8806, and it would collect parallel rays at the distance of 1226 thousandths of an inch: but the distance of the retina from the crystalline is 550 thousandths, and that of the anterior surface of the cornea 250; hence (by SMITH, Art. 367,) the focal distance of the cornea and aqueous humour alone must be 2329. Now, supposing the crystalline to assume a spherical form, its diameter will be 642 thousandths, and its focal distance in the eye 926. Then, disregarding the thickness of the cornea, we find (by SMITH, Art. 370,) that such an eye will collect those rays on the retina, which diverge from a point at the distance of 12 inches and 8 tenths. This is a greater change than is necessary for an ox's eye, for if it be supposed capable of distinct vision at a distance somewhat less than 12 inches, yet it probably is far short of being able to collect parallel rays. The human crystalline is susceptible of a much greater change of form.

The ciliary zone may admit of as much extension as this diminution of the diameter of the crystalline will require; and its elasticity will assist the cellular texture of the vitreous humour, and perhaps the gelatinous part of the crystalline, in restoring the indolent form.

It may be questioned whether the retina takes any part in supplying the lens with nerves; but, from the analogy of the olfactory and auditory nerves, it seems more reasonable to suppose that the optic nerve serves no other purpose than that of conveying sensation to the brain.

Although a strong light and close examination are required, in order to see the fibres of the crystalline in its intire state, yet their direction may be demonstrated, and their attachment shewn, without much difficulty. In a dead eye the tendons are discernible through the capsule, and sometimes the anterior ones even through the cornea and aqueous humour. When the crystalline falls, it very frequently separates as far as the centre into three portions, each having a tendon in its middle. If it be carefully stripped of its capsule, and the smart blast of a fine blow-pipe be applied close to its surface in different parts, it will be found to crack exactly in the direction of the fibres above described, and all these cracks will be stopped as soon as they reach either of the tendons. The application of a little ink to the crystalline is of great use in shewing the course of the fibres.

When first I observed the structure of the crystalline, I was not aware that its muscularity had ever been suspected. We have, however, seen that DESCARTES supposed it to be of this nature; but he seems to think that the accommodation of the eye to a small distance is principally performed by the elongation of the eye's axis. Indeed as a bell shakes a steeple, so must the coats of the eye be affected by any change in the crystalline; but the effect of this will be very inconsiderable; yet, as far as it does take place, it will co-operate with the other change.

But the laborious and accurate LEEUWENHOEK, by the help of his powerful microscopes, has described the course of the fibres of the crystalline, in a variety of animals; and he has even gone so far as to call it a muscle*; but no one has pur-

* Now if the cristaline humour (which I have sometimes called the crist. muscle)

sued the hint, and probably for this reason, that from examining only dried preparations, he has imagined that each coat consists of circumvolutions of a single fibre, and has intirely overlooked the attachment of the fibres to tendons; and if the fibres were continued into each other in the manner that he describes, the strict analogy to muscle would be lost, and their contraction could not have that effect on the figure of the lens, which is produced by help of the tendons. Yet notwithstanding neither he, nor any other physiologist, has attempted to explain the accommodation of the eye to different distances by means of these fibres, still much anatomical merit must be allowed to the faithful description, and elegant delineation, of the crystallines of various animals, which he has given in the Philosophical Transactions, Vol. XIV. p. 780, and Vol. XXIV. p. 1723. It appears, from his descriptions and figures, that the crystalline of hogs, dogs, and cats, resembles what I have observed in oxen, sheep, and horses; that in hares and rabbits, the tendons on each side are only two, meeting in a straight line in the axis; and that in whales they are five, radiated in the same manner as where there are three. It is evident that this variety will make no material difference in the action of the muscle. I have not yet had an opportunity of examining the human crystalline, but from its readily dividing into three parts, we may infer that it is similar to that of the ox. The crystalline in fishes being spherical, such a change as I attribute to the lens in quadrupeds cannot take place in that class of animals.

It has been observed that the central part of the crystalline

in our eyes, &c. Phil. Trans. Vol. XXIV. p. 1729.— *Crystallinum musculum, alias humorem crystallinum dictum, &c.* LEEUWENH. *op. omn.* I. p. 102.

becomes rigid by age, and this is sufficient to account for presbyopia, without any diminution of the humours; although I do not deny the existence of this diminution, as a concomitant circumstance.

I shall here beg leave to attempt the solution of some optical queries, which have not been much considered by authors.

1. MUSSCHENBROEK asks, What is the cause of the lateral radiations which seem to adhere to a candle viewed with winking eyes? I answer, the most conspicuous radiations are those which, diverging from below, form, each with a vertical line, an angle of about seven degrees; this angle is equal to that which the edges of the eyelids when closed make with a horizontal line; and the radiations are evidently caused by the reflection of light from those flattened edges. The lateral radiations are produced by the light reflected from the edges of the lateral parts of the pupillary margin of the uvea, while its superior and inferior portions are covered by the eyelids. The whole uvea being hidden before the total close of the eyelids, these horizontal radiations vanish before the perpendicular ones.

2. Some have inquired, Whence arises that luminous cross, which seems to proceed from the image of a candle in a looking-glass? This is produced by the direction of the friction by which the glass is polished: the scratches placed in a horizontal direction, exhibiting the perpendicular part of the cross, and the vertical scratches the horizontal part, in a manner that may easily be conceived.

3. Why do sparks appear to be emitted when the eye is rubbed or compressed in the dark? This is MUSSCHENBROEK's

fourth query. When a broadish pressure, as that of the finger, is made on the opaque part of the eye in the dark, an orbicular spectrum appears on the part opposite to that which is pressed: the light of the disc is faint, that of the circumference much stronger; but when a narrow surface is applied, as that of a pin's head, or of the nail, the image is narrow and bright. This is evidently occasioned by the irritation of the retina at the part touched, referred by the mind to the place from whence light coming through the pupil would fall on this spot; the irritation is greatest where the flexure is greatest, viz. at the circumference, and sometimes at the centre, of the depressed part. But in the presence of light, whether the eye be open or closed, the circumference only will be luminous, and the disc dark; and if the eye be viewing any object at the part where the image appears, that object will be totally invisible. Hence it follows, that the tension and compression of the retina destroys all the irritation, except that which is produced by its flexure; and this is so slight on the disc, that the apparent light there is fainter than that of the rays arriving at all other parts through the eyelids. This experiment demonstrates a truth, which may be inferred from many other arguments, and is indeed almost an axiom, viz. that the supposed rectification of the inverted image on the retina does not depend on the direction of the incident rays. NEWTON, in his sixteenth query, has described this phantom as of pavonian colours, but I can distinguish no other than white; and it seems most natural that this, being the compound or average of all existing sensations of light, should be produced when nothing determines to any particular colour. This average seems to resemble the middle form, which Sir JOSHUA REY-

NOLDS has elegantly insisted on in his discourses; so that perhaps some principles of beautiful contrast of colours may be drawn from hence, it being probable that those colours which together approach near to white light will have the most pleasing effect in apposition. It must be observed, that the sensation of light from pressure of the eye subsides almost instantly after the motion of pressure has ceased, so that the cause of the irritation of the retina is a change, and not a difference, of form; and therefore the sensation of light appears to depend immediately on a minute motion of some part of the optic nerve.

If the anterior part of the eye be repeatedly pressed, so as to occasion some degree of pain, and a continued pressure be then made on the sclerotica, while an interrupted pressure is made on the cornea; we shall frequently be able to observe an appearance of luminous lines, branched, and somewhat connected with each other, darting from every part of the field of view, towards a centre a little exterior and superior to the axis of the eye. This centre corresponds to the insertion of the optic nerve, and the appearance of lines is probably occasioned by that motion of the retina which is produced by the sudden return of the circulating fluid, into the veins accompanying the ramifications of the arteria centralis, after having been detained by the pressure which is now intermitted. As such an obstruction and such a re-admission must require particular circumstances, in order to be effected in a sensible degree, it may naturally be supposed that this experiment will not always easily succeed.

Fig. 1.

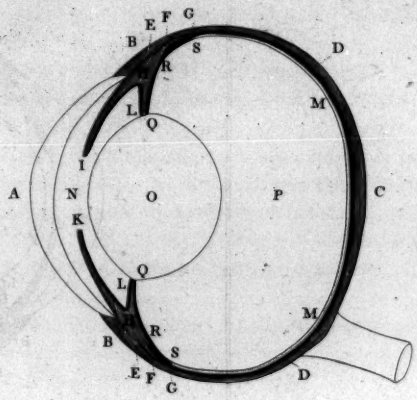


Fig. 2.

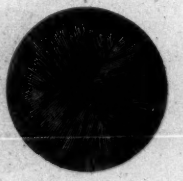
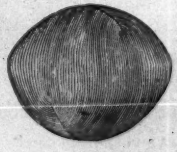


Fig. 3.



Explanation of the Figures.

Tab. XX. fig. 1. A vertical section of the ox's eye, of twice the natural size.

A. The cornea, covered by the tunica conjunctiva.

BCB. The sclerotica, covered at BB by the tunica albuginea, and tunica conjunctiva.

DD. The choroid, consisting of two laminas.

EE. The circle of adherence of the choroid and sclerotica.

FG, FG. The orbiculus ciliaris.

HI, HK. The uvea: its anterior surface the iris; its posterior surface lined with pigmentum nigrum.

IK. The pupil.

HL, HL. The ciliary processes, covered with pigmentum nigrum.

MM. The retina.

N. The aqueous humour.

O. The crystalline lens.

P. The vitreous humour.

QR, QR. The zona ciliaris.

RS, RS. The annulus mucosus.

Fig. 2. The structure of the crystalline lens, as viewed in front.

Fig. 3. A side view of the crystalline.

XVII. *Observations on a Current that often prevails to the Westward of* Scilly; *endangering the Safety of Ships that approach the* British *Channel. By* James Rennell, *Esq. F. R. S.*

Read June 6, 1793.

It is a circumstance well known to seamen, that ships, in coming from the Atlantic, and steering a course for the British channel, in a parallel somewhat to the *south* of the Scilly Islands; do, notwithstanding, often find themselves to the *north* of those islands: or, in other words, in the mouth of the St. George's, or of the Bristol channel. This extraordinary error has passed for the effects, either of bad steerage, bad observations of latitude, or the indraught of the Bristol channel: but none of these account for it satisfactorily; because, admitting that at times there may be an indraught, it cannot be supposed to extend to Scilly; and the case has happened in weather the most favourable for navigating, and for taking observations. The consequences of this deviation from the intended track, have very often been fatal: particularly in the loss of the Nancy packet, in our own times; and that of Sir Cloudesley Shovel, and others of his fleet, at the beginning of the present century. Numbers of cases, equally melancholy, but of less celebrity, have occurred; and many others, in which the danger has been imminent, but not fatal, have scarcely reached the public ear. All of these have been

referred to accident; and therefore no attempt seems to have been made, to investigate the cause of them.

I am however of opinion, that they may be imputed to a specific cause; namely, a current: and I shall therefore endeavour to investigate both that, and its effects; that seamen may be apprized of the times, when they are particularly to expect it, in any considerable degree of strength; for then only, it is likely to occasion mischief; the current that prevails at ordinary times, being, probably, too weak to produce an error in the reckoning, equal to the difference of parallel, between the south part of Scilly, and the track that a commander, prudent in his measures, but unsuspicious of a current, would chuse to sail in.*

It seems to be generally allowed, that there is always a current, setting round the Capes of Finisterre, and Ortegal, into the Bay of Biscay. This I have the authority of Captain MENDOZA RIOS, a Fellow of the Royal Society, and an officer in the royal navy of Spain, for asserting. Besides, such an intimation was amongst the earliest notices that I received, concerning matters of navigation, when on board of a ship that sailed close along the north coast of Spain, in 1757. The current then, is admitted to set to the eastward, along the coast of Spain; and continues its course, as I am assured, along the coast of France, to the north, and north-west: and indeed, any body of water, once set in motion, along a coast, cannot suddenly stop; nor does it, probably, lose that motion, until

* It may be remarked, by the way, that the true latitude of the present light-house on St. Agnes's Island, is 49°, 54′; and that of the most southerly part of the whole group of islands and rocks, is 49°, 52′. This is according to an advertisement given out by the Trinity House, in 1792.

by degrees it mixes with the ocean; after being projected into it, either from the side of some promontory, that extends very far beyond the general direction of the coast; or after being conducted into it, through a strait.

The original cause of this current, I apprehend to be, the prevalence of westerly winds in the Atlantic; which, impelling the waters along the north coast of Spain, occasions a current, in the first instance. The stronger the wind, the more water will be driven into the Bay of Biscay, in a given time; and the longer the continuance of the wind, the farther will the vein of current extend.

It seems to be clearly proved, that currents of water, after running along a coast that suddenly changes its direction, (as happens on the French coast, at the promontory south of Brest) do not change their course with that of the shore, but preserve, for a considerable time, the direction which they received from the coast they last ran by. In some instances, after being projected into the sea, they *never* again approach the shore; but preserve, to a very great distance, nearly the direction in which they were projected; as well as a considerable degree of their original velocity, and temperature. The gulf stream (of Florida) is a wonderful instance of this kind; which, originating in a body of *pent-up* waters, in the Gulf of Mexico, is discharged with such velocity, through the Straits of Bahama, that its motion is traceable through the Atlantic, to the Bank of Newfoundland; and may possibly extend much farther. This being therefore the case, we can have no difficulty in conceiving, that the current of the Bay of Biscay continues its course, which may be about NW.by W, from the coast of France, to the westward of Scilly and Ireland.

At ordinary times, its strength may not be great enough to preserve its line of direction, across the mouth of the British Channel; or, if it does preserve its direction, it may not have velocity enough to throw a ship so far out of her course, as to put her in danger. But, that a current prevails *generally*, there can be little doubt; and its degree of strength will be regulated by the state of the winds. After a long interval of moderate westerly gales, it may be hardly perceptible; for a very few miles of northing, in the 24 hours, will be referred to bad steerage, or some other kind of error: but after hard and continued gales from the western quarter, the current will be felt in a considerable degree of strength; and not only in the parallel of Scilly, but in that of the south-west coast of Ireland likewise.

Our observation of what passes in the most common waters, is sufficient to shew how easily a current may be induced, by the action of the wind, on the water contiguous to a bank, when the wind blows *along* it. In a canal of about four miles in length, the water was kept up *four inches* higher at one end, than at the other, by the mere action of the wind, along the canal. This was an experiment made, and reported to me, by my much lamented acquaintance, the late Mr. SMEATON. We know also, the effects of a strong south-west, or north-west, wind, on our own coasts: namely, that of raising very high tides in the British Channel, or in the Thames, and on the eastern coasts; as those winds respectively blow: because the water that is accumulated, cannot escape quick enough, by the Strait of Dover, to allow of the level being preserved. Also, that the Baltic is kept up *two feet* at least, by a strong NW wind of any continuance: and

that the Caspian Sea is higher by *several* feet, at either end, as a strong northerly, or southerly, wind prevails. Therefore, as water pent up, in a situation from which it *cannot escape*, acquires a higher level, so, in a place where it *can escape*, the same operation produces a current: and this current will extend to a greater or less distance, according to the force with which it is set in motion; or, in other words, according to the height at which it is kept up, by the wind.

It may possibly be asked, why a similar current does not prevail in the British Channel, from the same westerly winds? To this I answer, that the increased height and velocity of the tides, during the prevalence of such winds, prove that a part, at least, of the same effect which happens in the Bay of Biscay, is produced in the Channel; and I have little doubt, that there is, in fact, a current also; but that, as it is blended with the common tide, the effect on the senses is lost: for it may appear only in the form of a *stronger* flood tide, or a *weaker* ebb, than at other times. Whereas the Bay, a wider space, and of a different form, allows a freer scope to the tides, than the British Channel does: it being high water nearly at the same time, all over the Bay; but varying in the Channel, at least five hours. And it may be concluded, from analogy, that the form of the Channel does not allow of the same effect being produced by the wind, on its included waters, as may be produced on those of the Bay: these meeting with an opposition, in the coast of France, the others having a partial exit, at the Strait of Dover: we may also conclude, that if no such phænomenon as a tide existed, a current, though less strong than in the Bay, would be perceived in the British Channel.

Of the Bay of Biscay it may be observed that, by reason of its form, and exposure to the reigning winds, which are often violent, and which pass over a vast expanse of water, there is no part of the ocean, familiarly known to us, whose circumstances are, in any degree, similar to it. It ought not therefore to surprize us, if we find that it differs, in any particular, from other seas. Seamen have remarked its uncommon degree of agitation, in stormy weather; but this has not, as far as I know, been properly accounted for. May it not be owing generally, to the same cause as that which produces the current? and at times, to the very current itself? With respect to the first—the waves of a deep bay or gulf, when the wind forces the water into it, will meet with a resistance in the land at the head of it, which must occasion a reverberation, that will render the surface of a great part of the gulf more unquiet, than when there is an opening at the end, to allow the undulatory motion a freer scope. What is said here, is exemplified on a small scale, by Mr. SMEATON's very ingenious manner of quieting Ramsgate harbour. (See his Tract on that harbour, page 45.) And with respect to the second cause—the effect of a current running to windward, in producing a short, hollow, and therefore dangerous, wave, is pretty well known. Accordingly, at seasons when the current runs strong, and the wind blows fresh from the north-west quarter, this cause must also contribute to the agitation of the waters, in the north part of the bay.*

* How far the reverberatory motion may extend, I know not: but it is certain that an undulatory motion impressed on the sea by the wind, will extend to a prodigious distance; and even into a region where a different wind prevails: as for instance, a swell raised by a strong gale, at south, or south-west, in the tract of variable winds,

It is quite uncertain at what interval of time, from the commencement of strong westerly gales, in the Atlantic and Bay of Biscay, the current may operate on the tracks of ships, near Scilly; for we are not possessed of the *data*, requisite for determining it. If we were to conceive a current, originating on the coast of Spain, and afterwards disturbing the courses of ships, on the west of Scilly and Ireland; this would require too much time, to agree with one of the instances which I mean to adduce: although it is probable, that this may be nearly the effect at ordinary times, and when the westerly winds blow moderately. But as, in one striking instance, it appears that the current operated in a very remarkable manner, on the ship's course, on the fourth day after the commencement of the gale, in the quarter where the ship was; the cause should rather be looked for, in the *sudden* and *great accumulation* of water, in the Bay of Biscay: otherwise, there is no accounting for the sudden appearance of the current. And the very act of accumulation, causing an indraught, there will consequently be a current round the Capes of Finisterre, and Ortegal, towards the Bay. Be the exact cause, however, what it may, it no doubt originates in the Bay, by the action of strong westerly winds: the prevalence of such winds, will therefore be the *signal* for the appearance of a current, between Ushant, and the south-west coast of Ireland: for though the cause can only be guessed at, the effect is too well ascertained, to remain in doubt.

I shall now adduce the facts, on which the idea of the existence of a current is founded.

has been felt, very far within the limits of the south-east trade wind, in the Indian Ocean.

In crossing the eastern part of the Atlantic, in the HECTOR East India ship, in 1778, we encountered, between the parallels of 42 and 49, very strong westerly gales; but particularly between the 16th and 24th of January, when, at intervals, it blew with uncommon violence. It varied two, or more, points, both to the north and south of west, but blew longest from the northern points; and it extended, as I afterwards learnt, from the coast of Nova Scotia, to that of Spain.

We arrived within 60 or 70 leagues of the meridian of Scilly, on the 30th of January, keeping between the parallels of 49 and 50; and about this time we began to feel a current, which set the ship to the north of her intended parallel, by near half a degree, in the interval between two observations of latitude; that is, in two days. And the wind, ever afterwards, inclining to the south, would not permit us to regain the parallel; for, although the northern *set* was trifling, from the 31st until we arrived very near Scilly; yet the wind, being both *scant* and *light*, we could never overcome the tendency of the current. Add to this, that the direction of the current, being much more *westerly* than *northerly*, we crossed it on so very oblique a course, that we continued in it a long time; and were driven, as it appears, near 30 leagues to the west, by it: for we had soundings in 73 fathoms, in the latitude of Scilly, and afterwards ran 150 miles, by the log, directly east, before we came the length of the islands. In effect, in running 120 miles, we shallowed the water, only nine fathoms.

We not only were sensible of the current, by the observations of latitude, but by *riplings* on the surface of the water, and by the direction of the lead line. The consequence of all

this was, that we were driven to the north of Scilly; and were barely able to lay a course through the passage between those islands and the Land's End.

Having no time keeper on board, we were unable to ascertain the several points, in this part of our track, and therefore can only approximate our longitude; and that but very coarsely. But according to what we learnt from our soundings, and from a vessel which had only just entered the current, it may be concluded, that the current, at times, extends to 60 leagues, west of Scilly; and also runs close on the west of those islands. However, the breadth of the stream, may probably be little more than 30 leagues; for we crossed it, as has been said, very obliquely; and perhaps, in the widest part.

The journal of the ATLAS East India ship, Captain COOPER, in 1787, furnishes much clearer proofs, both of the existence of the current, and of the rate of its motion: for having time keepers on board, Captain COOPER was frequently enabled to note the difference between the true, and the supposed, longitude; and it may be said, that this journal, by the means it affords of ascertaining the current, is highly valuable; as containing some very important facts, and which might have been entirely lost to the public, had not Captain COOPER marked them, in the most pointed manner.

I shall proceed to state, in abstract, the most important of the facts recorded in the journal.

The ATLAS sailed with a fair wind, and took her departure from the Isle of Wight, on the 25th of January, 1787; and on the 27th had advanced 55 leagues to the westward of Ushant; when a violent gale of wind began at south, and,

about 11 hours afterwards, changed suddenly to the westward. The gale continued through the four following days: on the 28th, it was generally W by S, and WSW; on the 29th, SW by W, or more southerly; and on the 30th and 31st, SSW, to SW by S.*

During this long interval, the ship was generally *lying to*; and with her head to the NW. On the 1st of February, the wind abated, but still blew from the south-westward; and the ship was kept to the north-west. The stormy weather returned again the following day, and continued, with little intermission, until the 11th; blowing from all the intermediate points, between south and WNW; but chiefly, and most violently, from the WSW, and SW. At intervals, on the 8th and 9th in particular, the journal remarks, that "*it blew a mere hurricane.*" On the 11th, the weather growing more moderate, and the wind favourable, the ship proceeded on her course, southward; being then two degrees and a quarter of longitude, to the west of Cape Finisterre, by the reckoning; but by the time keepers, more than *four degrees and a half*.

After the above abstract of the proceedings of the ship, I shall subjoin the following particulars; which are the most in point, to the purpose of the present discussion.

On the 27th, at noon, soon after the gale commenced, the longitude, by reckoning, agreed within 14 minutes of that shewn by the time keepers; the latter being the most westerly. This difference alone might well have arisen from

* In this, as well as in the former statement of the winds, I have allowed for the variation of the compass; that the application of it, to the quarter of the heavens, and to the chart annexed, (see Tab. XXI.) may be more easy and clear.

an error in the log, or even in the position of the needle point on the Isle of Wight, from whence the departure was taken; but it may also be owing to the westerly current, whilst the ship remained in it, on the 27th; if we admit that such a current prevails at all times, though in different degrees of strength. Here it is proper to remark, that in delineating Captain COOPER's track, on the chart, I have scrupulously adhered to the result of each day's work, of the reckoning, as I find it in his journal; contenting myself with inserting my own observations on the track, in this paper only; where they cannot mislead.

The longitudes pointed out by the time keepers on the 28th, 29th, and 30th, shew, that the increasing, though trifling differences, between the true longitude, and that by the dead reckoning, had amounted to 24 minutes only, on the 30th. At this time the ship was about 24 leagues to the WSW of Scilly; and, at 5 or 6 leagues to the SSE of this position, (that is, at 25 leagues SW by W from Scilly) they had soundings at 70 fathoms. This last particular is mentioned, to prove that the longitude shewn by the time keepers (8° 28′ west from London) was nearly the longitude in which the ship really was, on the 30th of January. That of St. Agnes (Scilly) is taken at 6° 46′.

The Atlas was now entered into the stream of the same current which occasioned so much delay to the Hector; but the course of the Atlas, being opposite to that of the Hector, it facilitated her progress; and also carried her clear of the south-west coast of Ireland.

On the 31st, the time keepers shewed that the ship had been *set* very considerably to the westward of the reckoning; and by

the 2d of February, at 3 in the afternoon, it appeared that she had been *set* two whole degrees of longitude to the west of the reckoning, since the 30th at noon; that is, in the course of 51 hours. (Here it may be proper to remark, that I have, throughout, reckoned according to *sea time;* that is, the day commences at noon.)

On the 3d of February, at noon, the time keepers shewed a further *set*, of 23 minutes of longitude, more than the reckoning gave, in the interval since the last observation, which was 45 hours; so that, since the 30th of January, 4 days only, the ship had been carried by the current, no less than two degrees and twenty-three minutes; and since the 27th, when the gale began, 2° 32′ of longitude; amounting, in these parallels, to ninety-nine marine miles. But here, the current appears to have totally left them; and it is very probable, that it even ceased before the time of observation, on the 3d: for the succeeding observations of the 5th, 6th, 7th, 9th, 10th, and 11th, although the strong westerly gales continued, come so near the longitude by the reckoning (deduced from the observation of the 3d) that the differences, which are sometimes to the east, and at other times to the west, may be with more propriety ascribed to errors of the log, than to a current; as may be seen by the two tracks on the chart. We may therefore conclude, that the current did not cease at the very point of time, when the observation of the 3d was taken, but probably some time before.

It appears then, that the Atlas experienced a westerly current, from a point about 24 leagues to the WSW of Scilly, (if not earlier) to four degrees of longitude west of the meridian

of Cape Clear,* in the parallel of 51°; where its effects were no longer perceptible. And, as no current was felt in the track southward, on the 11th; nor in any part of the track to the north-west, between the 3d and 10th; although it was felt nearly in the same line of direction, between the 1st and 3d; it may be inferred that the stream goes off to the north-west, between the aforesaid track, and the south-west coast of Ireland. It is much to be regretted that no observations appear on the 12th and 13th; which would have been decisive of its course.

I come now to two particulars of the case, which, I confess, perplex me exceedingly. The first is, that the current was felt, apparently in its full strength, on the fourth day after the commencement of the gale; which began at south, then changed suddenly to the west and WSW, and afterwards fixed in the SW quarter. This gale was felt between the 48th and 50th degrees of latitude, and, no doubt, extended its effects very far to the south and west; but what the state of the winds had been in those quarters, previous to the 27th of January, we are ignorant. The winds in the British Channel had been easterly, for three days preceding the gale: the fourth day, preceding, there had been strong gales at SW; and the five days preceding *that*, there had been chiefly light winds at west. According to this state of facts, we can only suppose that the current originated from a vast body of water, pent up in the Bay of Biscay, by violent gales of wind; first from the southward, eleven hours; then from a point or two to the south of west; and lastly, at south-west.

* Cape Clear is reckoned to be in long. 9° 25′ from London.

We are not to consider the water of this current, as having made the circuit of the Bay of Biscay; but as the *collective body* of *pent up* waters, in the Bay, running off along the SW coast of Britanny, and thence to the north-westward; preserving nearly the direction it had acquired, by running along that coast. And it may be conceived, that the frequent recurrence of westerly winds, keeps up a constant current in the Bay, and to some distance beyond it; although during the longest intermissions of these winds, the current may become so slow, as to be scarce perceptible.

The second particular which perplexes me, is, that no northern *set* is indicated by Captain COOPER's journal: that is to say, by the mode in which each day's log is wrought; and which, in the formation of the chart, as is said before, I have strictly adhered to. It indeed appears to me very wonderful, that no northing should appear, when it seems to be the very same kind of current which carried the Hector so far to the northward. It is certain, that the state of the weather was such, as to preclude those nice attentions to the reckoning, which might enable us to detect any small differences, between the latitude by account, and that by observation; although the western *set* was too considerable to escape notice, and may even have been more than the statement sets forth. I cannot therefore, by any means admit, that there was no northing in the current through which the Atlas passed; first, because they had not observations of latitude, regularly; and lastly, because on the 31st of January, when *lying to*, 36 miles are allowed for 20 hours *drift*, to the north-west; which appears to me excessive. On that day they had no observation of latitude, and on the following

day, the observation shewed two miles northing; which however proves nothing. Again, on the succeeding day, (the 2d) in a most important point of the track, there was no observation of latitude.

In the Hector, precisely in the same track, and at the same season of the year, the current had, as has been observed, a considerable degree of northing in its course. On two days it was about 12 miles, each; on another day 13, and on two others, 9, and 8; and this, in weather very favourable for keeping a reckoning, and with observations of latitude, on every day save one; not to mention the strong circumstances of a visible *set* to the northward, indicated, as well by the lead line, as by the ripling on the surface of the water. It is in the nature of currents, to expand their streams or columns of water, after being projected into the ocean; and therefore, according to this law, the middle part of the stream should preserve its original course, in a greater degree than the borders of it; so that the middle part may run to the NW by W, whilst the eastern border may run more *northerly*, and the western border more *westerly*. It is certain, that in the Hector, we felt the northerly current much stronger, close on the west of Scilly, than further out; and it appeared by the distance we ran, after sounding in 73 fathoms, that the current must have set much more *westerly*, than *northerly*, the whole time.

The following remarks obviously occur, on the effect of this current.

1st. Whatever may be the breadth of the stream, (which is at present unknown) if a ship crosses it *very obliquely*, that is, in an E by S, or more southerly direction (as may easily

happen, on finding herself too far to the northward, at the first place of observation, after she gets into the current), she will, of course, continue much longer in it, and will be more affected by it, than if she steered more directly across it. She will be in a similar situation, if she crosses it with light winds; and both of these circumstances should be attended to. And if it be true, as I suspect it is, that the eastern border of the current has a more northerly direction than the middle of it, this also should be guarded against. I conceive also, that the stream is broader in the parallel of Scilly, than farther south. And here we may remark, that those who, from a parallel south of Scilly, have been carried clear of it to the north, when approaching it, in the night, may esteem themselves fortunate that the current was *so strong*; for had it been weaker, they might have been carried on the rocks.

2d. A good observation of latitude, at noon, would be thought a sufficient warrant for running eastward, during *a long night*: yet as it may be possible to remain in the current, long enough to be carried from a parallel that may be deemed a very safe one, to that of the rocks of Scilly, in the course of such a night; it would appear prudent, after experiencing a continuance of strong westerly gales in the Atlantic, and approaching the Channel with light southerly winds, either to make Ushant, or at all events to keep in the parallel of 48°, 45′, at the highest. If they keep in 49°, 30′, they will experience the whole effect of the current, in a position where they can least remedy the evil: but if in 48°, 45′, they are assailed by the north-west current, they are still in a position from whence a southerly wind will carry them into the

Channel. But all ships that cross the Atlantic, and are bound to the eastward of the Lizard, had better make Ushant, under the above circumstances, in times of peace. Or, at all events, why should they run in a parallel, in which they are likely to lose ground?

8d. Ships, bound to the westward, from the mouth of the Channel, with the wind in the south-west quarter, so that it may appear indifferent which tack they go on, should prefer the *larboard* tack; as they will then have the benefit of the current.

4th. I understand that the light house of Scilly is either removed, or to be removed, to the south-west part of the islands; or of the high rocks. This is certainly a wise measure; as the light should be calculated more particularly for ships that have a *long*, than a *short* departure; like those from any part of the European coasts, to the northward, or eastward. The light house ought also to be built very lofty. I am sorry to remark, that, as far as my observation has gone, this light has never appeared clear and bright, as a light to direct ships ought to do.

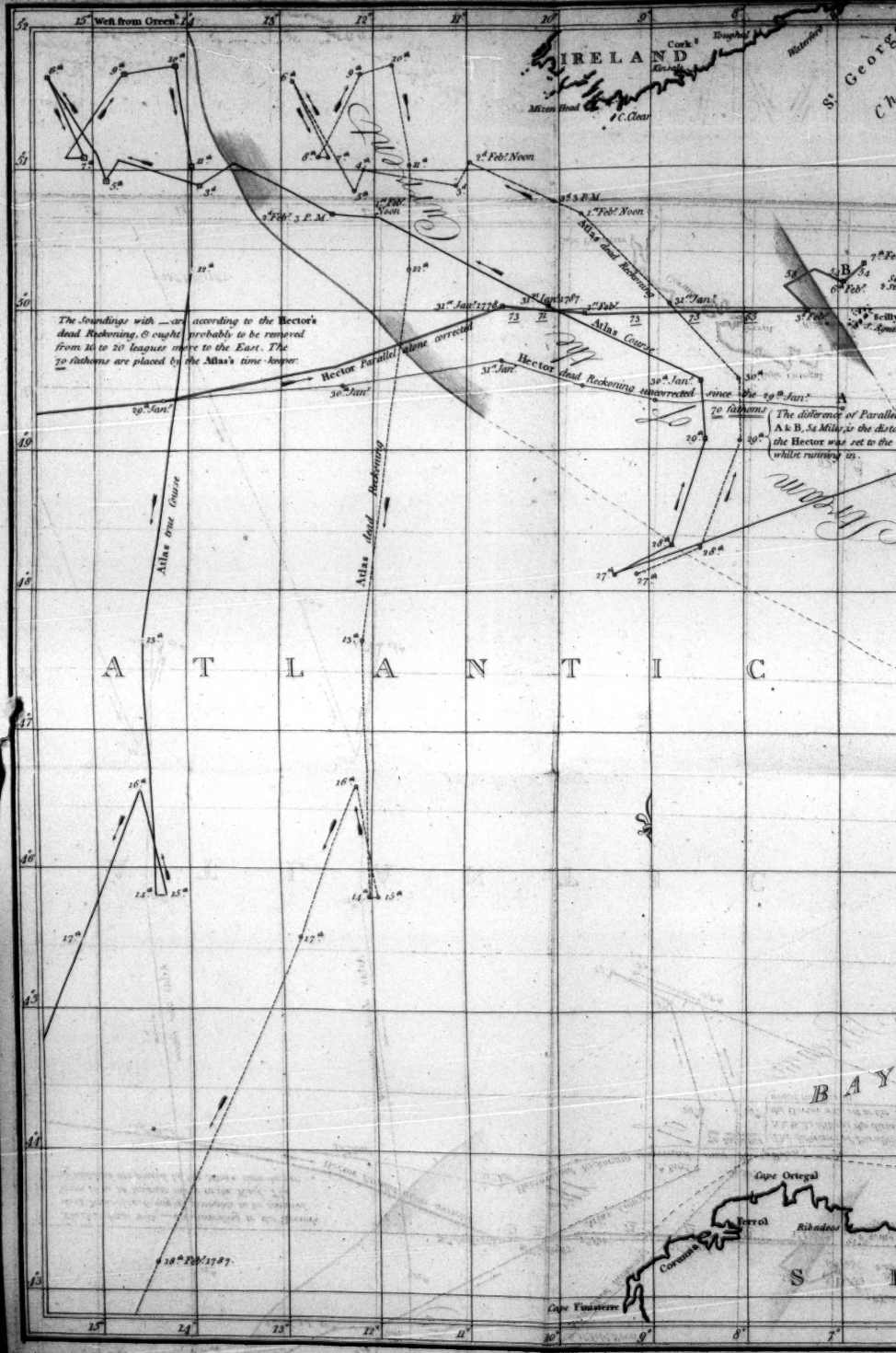
5th. It would be worth the attention of government (in my humble opinion) to send a vessel with time keepers on board, in order to examine and note the soundings between the parallels of Scilly and Ushant, at least; from the meridian of the Lizard point, as far west as the moderate depths extend; I mean such as can be ascertained with exactness, in the ordinary method of sounding. I have reason to suppose that our chart of soundings is very bad; and indeed, how can it be otherwise, considering the imperfect state of the art of marine surveying, at the time when it was made? A set of time

keepers will effect more, in the course of a summer, in the hands of a skilful practitioner, than all the science of Dr. HALLEY, during a long life; for who could place a single cast of soundings, in the open sea, without the aid of a time keeper? The current in question, must have disturbed every operation of this kind. It should be the task of the person, so employed, to note all the varieties of bottom, as well as the depths; the time of high and low water; setting of the tides, and currents, &c. Such a survey, skilfully conducted, might enable mariners to supply the want of observations of latitude, and of longitude; and, of course, to defy the current, as far as relates to its power of misleading them.

6th. It is certain, that the current in question may be somewhat disturbed by, or rather will appear to be blended with, the tides, at the entrances of the British and St. George's Channels; but it is obvious that the current will have the same effect, in setting a ship out of her course, as if no tide existed; because, whatever effect one tide may have, the next will nearly do away. But there are two particulars, well worth ascertaining; and these are, first, the point at which the two tides of St. George's, and of the British Channel separate, on the west of Scilly. And secondly, what degree of northing one of the streams has, more than the other. Because a ship, in approaching Scilly, from the west, on a flood tide, and keeping in a parallel which may be to the *north* of the point of separation of the two tides, (and consequently in the tide stream of St. George's Channel) may be thrown too far to the north; although, had she been far enough to the west, to receive the effect of the next ebb, this temporary, and alternate derangement of the course, would

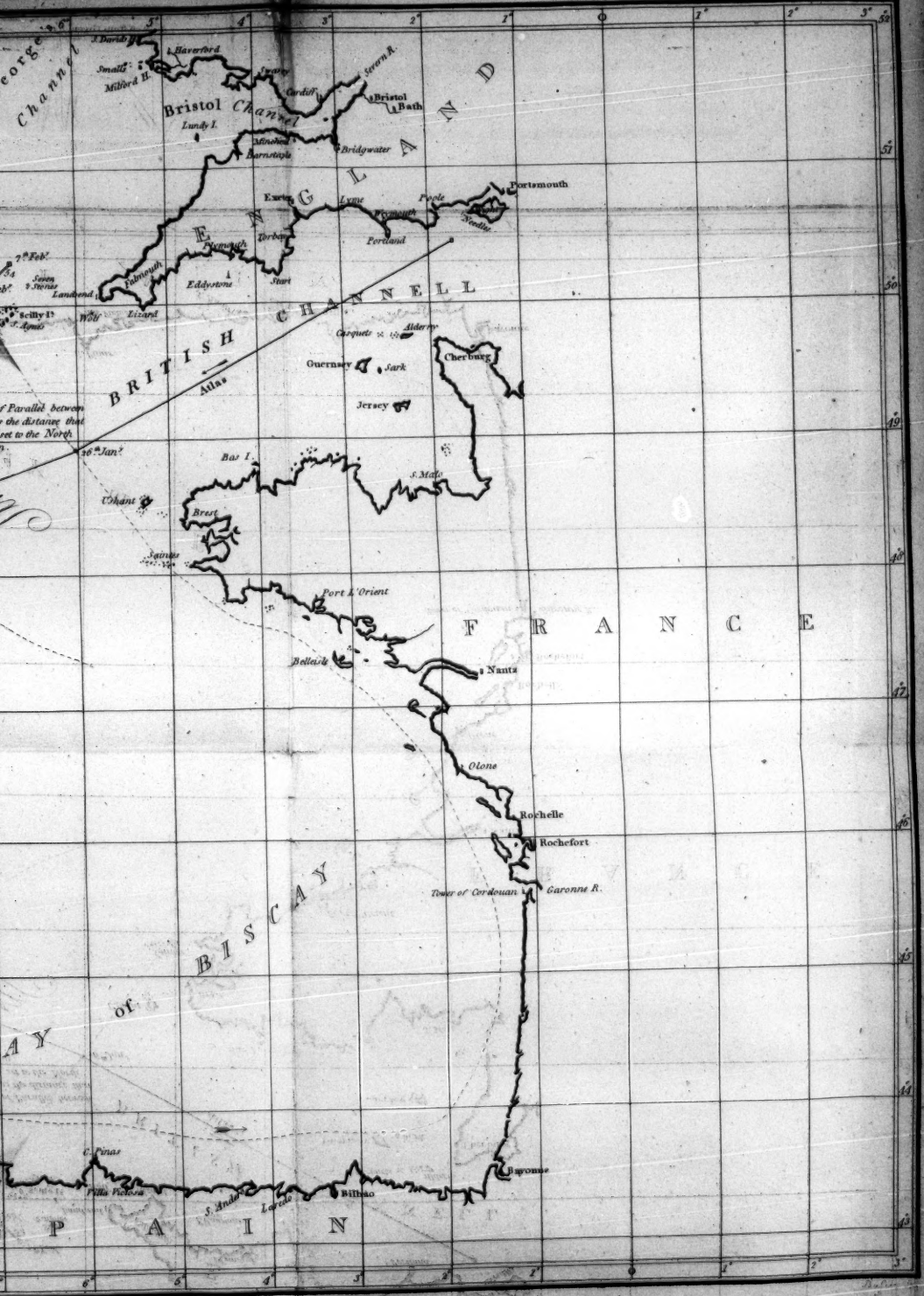
have had no ill effect; or even have been noticed. But admitting that a tide, with any degree of northing in it, does take place, a little to the west of Scilly; this will furnish an additional reason for keeping in a southern parallel.

CHART of the *TRACKS* of the HECTOR and ATLAS *EAST INDIA SHIPS*, in 1778 & 1787.


IRELAND
Cork
Waterford
Mizen Head
C. Clear
St. George's Ch
West from Greenw.
The Soundings with — are according to the Hector's dead Reckoning, & ought probably to be removed from 10 to 20 leagues more to the East. The 70 fathoms are placed by the Atlas's time-keeper.
Hector Parallel
Atlas Course
Hector dead Reckoning uncorrected
70 fathoms
Atlas true course
Atlas dead Reckoning
A T L A N T I C
B A Y
Cape Ortegal
Ferrol
Corunna
Cape Finisterre
18th Feb.y 1787

17. Exhibited with a Design to pro[ve] the Existence of a CURRENT, between USHANT and IRELAND.

Philos. Trans. MDCCXCIII. Tab. XXI. p. 100.



XVIII. *Observations on the Planet Venus.* By William Herschel, *LL. D. F. R. S.*

Read June 18, 1793.

The planet Venus is an object that has long engaged my particular attention. A series of observations upon it, which I began in April, 1777, has been continued down to the present time.

My first view, when I engaged in the pursuit, was to ascertain the diurnal rotation of this planet; which, from the contradictory accounts of Cassini and Bianchini, the former of which states it at 23 hours, while the latter makes it 24 days, appeared to me to remain unknown, as to its real duration: for the observations of these gentlemen, how widely different soever with regard to time, can leave no doubt but that this planet actually has a motion on its axis.

The next object was the atmosphere of Venus; of the existence of which also, after a few months observations, I could not entertain the least doubt.

The investigation of the real diameter, was the third object I had in view.

To which may be added, in the last place, an attention to the construction of the planet, with regard to permanent appearances; such as might be occasioned by, or ascribed to, seas, continents, or mountains.

The result of my observations would have been communicated long ago, if I had not still flattered myself with the hopes of some better success, concerning the diurnal motion of Venus; which, on account of the density of the atmosphere of this planet, has still eluded my constant attention, as far as concerns its period and direction. Even at this present time, I should hesitate to give the following extract from my journals, if it did not seem incumbent upon me to examine by what accident I came to overlook mountains in this planet, which are said to be "*of such enormous height, as to exceed four, five, and even six times the perpendicular elevation of Cimboraco, the highest of our mountains!*"*

The same paper, which contains the lines I have quoted, gives us likewise many extraordinary relations, equally wonderful; such as hints of the various and singular properties of the atmosphere of Saturn.† A ragged margin in Venus, resembling the uneven border of the moon, as it appears to a power magnifying from 1 to 4.‡ One cusp of Venus appearing pointed, and the other blunt, owing to the shadow of some mountain.§ Flat spherical forms conspicuous on Saturn.‖ All which being things of which I have never taken any notice, it will not be amiss to shew, by what follows, that neither want of attention, nor a deficiency of instruments, could occasion my not perceiving *these mountains of more than 23 miles in height*;** *this jagged border of Venus*; *and these flat spherical forms on Saturn.*

* See Phil. Trans. for 1792, Part II. page 337. † Ibidem, p. 309. ‡ p. 310. § p. 312. ‖ p. 336. ** The height of Chimbo-raço, according to Mr. CONDAMINE, is 3200 French toises: and the English mile, by Mr. DE LA LANDE, measures 830. If the mountains in Venus exceed Chimbo-raço six times in perpendicular elevation, they must be more than 23 miles in height.

Indeed with regard to Saturn, I cannot hesitate a single moment to say, that, had any such things as flat spherical forms existed, they could not possibly have escaped my notice, in the numberless observations with 7, 10, 20, and 40-feet reflectors, which I have so often directed to that planet. However, if the gentleman who has seen the mountains in Venus, has made observations on flat spherical forms on Saturn, it is to be regretted that he has not attended to the revolution of this planet on its axis, which could not remain an hour unknown to him when he saw these forms.

Last night,* for instance, I saw two small dark spots on Jupiter; I shall not call them flat spherical forms, because their flatness, as well as their sphericity, must be hypothetical; moreover, these two terms seem to me to contradict each other. These were evidently removed, in less than an hour, in such a manner as to point out, very nearly, the direction and quantity of the rotation of this planet.

Before I remark on the rest of the extraordinary relations above-mentioned, I will give a short extract of my observations on Venus, with such deductions as it seems to me that we are authorised to make from them.

Observations.

April 17, 1777. The disk of Venus was exceedingly well defined, distinct, and bright, but no spot was visible by which I could judge of her diurnal motion. The same telescope shews the spots on Mars extremely well. 7-feet reflector.

April 26, 1777. The disk well defined, and bright, but no spot. 10-feet reflector.

* May 31, 1793.

February 21, 1780. No spot on the disk of Venus; diameter 15″,9, mean of three measures.

May 2, 1780. No spot; power 449; diameter 17″,2.

May 28, 1780. No spot; power 268 and 449; diameter 22″,8.

May 29, 1780. I viewed Venus with a 20-feet Newtonian reflector; power 447. The edge of the disk was so sharp and well defined, that there can be no wish to see it better. There was no spot of any kind.

I could see no projections of any mountains, though the phase of Venus is now such as would be most favourable for shewing them.

June 19, 1780. There is, on Venus, a bluish, darkish spot, *a d c*; and another, which is rather bright, *c e d*; they meet in an angle at *c*, the place of which is about one-third of the diameter of Venus from the cusp *a*. See Tab. XXII. fig. 1.

June 21, 23, 24, 25, 26, 28, 29, 30, and July 3, 1780. Continued observations were made upon these, and other faint spots, and drawings of them annexed. The instrument I used was a 20-feet Newtonian reflector, furnished with no less than five different object specula, some of which were in the highest perfection of figure and polish; the power generally 300 and 450. But the result of them would not give me the time of the rotation of Venus. For the spots assumed often the appearances of optical deceptions, such as might arise from prismatic affections; and I was always very unwilling to lay any stress upon the motion of spots, that either were extremely faint and changeable, or whose situation could not be precisely ascertained.

However, that Venus has a motion on an axis cannot be

doubted from these observations; and that she has an atmosphere is as evident, from the changes I took notice of, which surely cannot be upon the solid body of the planet.

Sept. 18, 1780. No spot on Venus; diameter 38″,4.

Oct. 10, 1780. With a very perfect 7-feet speculum; power 227, 460, and 932. No spot visible; diameter 41″,3.

Oct. 11, 1780. No spot; diameter 27″,8.

Oct. 20, 21, 23, 1780. No spot visible.

April 17, 1783. 10-feet reflector; power 324. I see some darkish spots on Venus. 7-feet reflector; power 227. The same appearances; but in neither of the instruments are they determined enough to serve for the purpose of finding the rotation.

May 21, 1783. 10-feet reflector; a new speculum; power 250.

7^h 30′. No spot visible.

8^h 30′. There seems to be an ill defined spot.

9^h 15′. No motion can be perceived that may be depended upon, though the figure seems rather advancing towards the centre.

May 30, 31, and June 1, 6, 1783. Spots were observed with 10 and 20-feet reflectors, and also motion perceived in them. Continued observations were recorded; and a great many figures delineated.

Dec. 3, 1783. With 460, and 932. No spot. No kind of protuberance, or indenture in the line which terminates the illumination, that might denote a mountain.

Feb. 13, 1785. No spot. A new 10-feet Newtonian reflector.

April 8, 1788. No spot on Venus; but she is still at too great a distance for such observations.

Nov. 30, 1789. No satellite visible. If she has one, it must be less in appearance than a star of the 8th or 9th magnitude; power 300.

Dec. 2, 1789. No spot; power 157, 300, and 460.

May 23, 1791. 40-feet reflector. The light of Venus is so brilliant that it becomes very uneasy for the eye to bear it long. There is no spot on the disk.

I had prepared my apparatus for a regular succession of observations with this instrument, having turned it towards the west, and put on the round-motion to keep the planet in view; but found that the great advantage of this telescope, which is its superior light, was, on this occasion, not only unnecessary, but rather an inconvenience.

Nov. 24, 1791. Correction of the clock, — 46″,7.

I took measures of the diameter of Venus with the 20-feet reflector; power 157.

19^h 18′	1st measure	45″,486
	2d ———	46 ,142
	3d ———	45 ,514
	4th ———	45 ,814
	5th ———	46 ,033
	6th ———	46 ,252
	Mean of the six measures	45″,874

I took five more, with a power of 300, the morning being very fine and clear.

12h 36′	1st measure	44″,885
	2d ——	45 ,705
	3d ——	45 ,104
	4th ——	45 ,322
	5th ——	45 ,842
	Mean of the five measures	45″,372
	Mean of the two sets	45″,623

These measures were taken with a speculum that has been lately re-polished, and therefore required new tables for casting them up. Such tables were made by the following transits.

Nov. 25, 1791. Transits of equatorial stars, taken to determine the value of the micrometer, which is divided into revolutions of sixty parts each.

First set, 23″,0 23,0 23,0 23,0 23,2 23,1 23,1 23,0 23,1 23,1 = 23″,06 = 21 revolutions; correction + 7,2 parts, for zero and concave wires.

Second set, 16″,8 16,6 16,4 16,5 16,7 16,6 16,5 16,8 16,4 16,5 = 16,58 = 15 revolutions 1,3 parts. Correction + 7,2.

By the first set,	1 part =	0″,272964
Second set	– –	273748
Mean of the two sets		0″,273356

In the first set, the micrometer was opened to 21 revolutions; and ten equatorial stars were observed to pass from one wire to the other. The opening was afterwards changed, and ten other stars were again observed to pass over the wires; after which the micrometer was read off, and found to be 15 revolutions and 1,3 parts.

Feb. 4, 1793. Correction of the clock, — 1′ 28″,0.

2^h 55′. 7-feet reflector; power 172. The air is very clear, and I see Venus very well defined; but cannot perceive any inequality on the edge of the planet that might denote a mountain; though the situation is favourable, being a little more enlightened than what we may call her last quarter. With 215, I had a very distinct view for a long time; but cannot perceive any inequality on the line which divides light from darkness.

With 287, I perceive no mountains: with 430, very distinct, I perceive no mountains. The terminating line is not so sharply defined as the circumference; but no inequality is visible.

With the same power, I see on Saturn, the equatorial belt, the shadow of the ring on Saturn, the shadow of Saturn on the ring, the division of the ring, &c.

I do not find any spot on Venus; so that there is no possibility to assign its diurnal motion.

March 3, 1793. Correction of the clock, — 2′ 0″,6.

6^h 30′. 7-feet reflector; I observed Venus with many powers, but could perceive no spot by which its diurnal motion might be ascertained.

April 3, 1793. Correction of the clock, — 2′ 48″,9.

9^h 9′. 7-feet reflector; power 215. The evening remarkably fine. There is no spot upon the disk of Venus, by which its rotation might be ascertained. The horns are equally sharp. There is nothing that has the appearance of a mountain, like what we see in the moon. With 287, very well defined, appearances are the same. With 430, not the least appearance of any mountains.

April 4, 1793. Correction of the clock, — 2′ 45″,3.

9ʰ 8′. There is no spot upon the disk of Venus. The horns are perfectly alike.

Not the least appearance like the mountains of the moon. With 287, and 430, very distinct.

April 5, 1793. Correction of the clock, — 2′ 46″,7.

8ʰ 25′. 7-feet reflector; power 215, 287, and 430. There are no spots upon Venus, by which its diurnal motion could be ascertained. The horns are exactly alike; and no inequality, like the mountains of the moon, is visible.

April 6, 1793. Correction of the clock, — 2′ 48″,1.

9ʰ 29′. With the 7-feet reflector; power 430. There is no kind of spot visible in any part of the disk. The two horns are exactly alike; and no appearance of mountains can be perceived.

April 7, 1793. Correction of the clock, — 2′ 49″,6.

9ʰ 8′. With the 7-feet reflector; power 215, 287, 430, and 860. I can see no spot upon the disk. Both horns are perfectly alike. Nothing resembling the mountains upon the moon can be perceived. I see it beautifully well, and sharply defined.

April 8, 1793. Correction of the clock, — 2′ 51″,0.

9ʰ 2′. With the 10-feet reflector; power 300, and 400. There is no spot upon Venus. The shape of the two horns is perfectly alike, and no appearance of mountains can be perceived. The illumination of the horns is also perfectly alike.

April 9, 1793. Correction of the clock, — 2′ 52″,1.

8ʰ 45′. With the 10-feet reflector; power 300. No spot upon Venus. Both horns perfectly alike. No appearance of mountains.

The light of Venus is brighter all around the limb, than on

that part which divides the enlightened, from the unenlightened part of the disk. With 400, appearances are the same.

9h 16′. The bright part, on the limb of Venus, is like a bright bead, of nearly an equal breadth all around.

April 16, 1793. Correction of the clock, — 2′ 59″,5.

10h 3′. 7-feet reflector, with different powers. No spot upon the disk. No mountains visible. Both horns alike.

A luminous margin, as usual, all around the limb.

April 20, 1793. Correction of the clock, — 3′ 3″,8.

10h 0′. 7-feet reflector; power 172, 215, 287, 430, and 860. No spot upon the disk. Both horns exactly alike. Not the least appearance of any mountains.

With 287, there is a narrow luminous border all around the limb, and the light afterwards diminishes pretty suddenly, and suffers no considerable diminution as we go towards the line which terminates the enlightened part of the disk. It is however less bright near the terminating line than farther from it. With powers lower than 287, the narrow luminous border cannot be so well distinguished.

April 22, 1793. Correction of the clock, — 3′ 5″,9.

9h 30′. 7-feet reflector; power 430. Very distinct. No spot. No appearance of mountains. Both horns perfectly alike.

With 860, 1290, and 1720, not the least appearance of mountains. Even the last power is considerably distinct.

10h 20′. With 430, the luminous margin, compared to the light adjoining to it, may be expressed by, *suddenly much brighter all around the limb.*

April 28, 1793. Correction of the clock, — 3′ 12″,8.

12h 0′. 7-feet reflector; power 215. No spot. Both horns perfectly alike. No appearance of mountains.

April 29, 1793. Correction of the clock, — 3′ 13″,4.

10^h 30′. 7-feet reflector; power 215. No spot. Both horns perfectly alike. Not the least appearance of any mountains.

With 287 and 430. Both horns equally sharp: no mountains visible.

May 1, 1793. Correction of the clock, — 3′ 15″,5.

10^h 45′. With the 10-feet reflector; power 300. No spot. Both horns perfectly alike, and very sharp. Not the least appearance of any mountains.

With 600, very distinct. Both horns extremely sharp, and alike. No mountains.

With 400, the same appearances.

May 5, 1793. Correction of the clock, — 3′ 19″,8.

11^h 27′. 7-feet reflector; power 215, 287, and 430. Both horns perfectly alike. No spot. Not the least appearance of any mountains.

May 12, 1793. Correction of the clock, — 3′ 27″,3.

11^h 10′. 7-feet reflector; power 215. Beautifully distinct. No spot visible; indeed the crescent is so slender, that we cannot expect to see any spots upon the disk.

Not the least appearance of any mountains, or inequality on the border.

The slender part of the crescent appears often knotty, but this is evidently a deception arising from undulations in the air; for, with proper attention, the knots may be perceived to change place. Little scratches in the great, or small speculum, may also occasion seeming irregularities; but, with proper attention, all such deceptions may be easily detected. Both horns perfectly alike.

With 287, 430, and 860, all that has been mentioned before is perfectly verified, and confirmed.

11^h 43′. I tried also the lower powers of 172, and 115; but they are inferior, in effect, to 215, 287, and 430; and not adequate to the delicacy and power required in such observations.

I have often taken notice, and again this evening, that the illuminated part of Venus is more than a semi-circle. Whether the excess of the sun's diameter alone will account for this, or how far we are to take the twilight of the atmosphere of Venus into consideration, I have hitherto deferred investigating, as my disk-micrometer wants a moveable parallel, in order to be adjustable, by observation, to the quantity of the horns which is enlightened beyond an hemisphere.

May 13, 1793. Correction of the clock, — 3′ 28″,4.

11^h 45′. 7-feet reflector; power 115, 172, 215, 287, and 430. Both horns perfectly alike. No appearance of mountains.

The points of the horns appear more blunt than they were last night, and are not drawn out to so slender a point; but this is evidently a deception, owing to the indifference of the night; for great sharpness, and distinct vision, are wanting in every other object I am looking at.

May 18. 1793. Correction of the clock, — 3′ 33″,7.

12^h 28′. 7-feet reflector; power 287. Both horns perfectly alike. No appearance of mountains. No spot. But, at the present altitude of Venus, it is impossible to make any observations that require delicacy, and demand very distinct vision with high powers.

May 19, 1793. Correction of the clock, — 3′ 34″,7.

11^h 45′. 7-feet reflector; power 287. Both horns perfectly alike, in shape and illumination. Not the least appearance of any mountains. The horns are exceedingly slender.

12^h 0′. I do not see any diminution of light on the edge of the horns, but what may be accounted for from their slenderness; being brought to very fine points, that lose themselves by their minuteness.

I saw it in great perfection, with a newly polished, plain speculum, which excels my former one in sharpness.

May 20, 1793. Correction of the clock, — 3′ 35″,8.

12^h 20′. No spot or unevenness in the light of Venus upon either cusp, or in any other part, that could in the least make me suspect a mountain.

I measured the diameter of Venus, and projection of the cusps beyond an hemisphere, by my disk-micrometer. This was not done by an illumination, as described in the apparatus, (Phil. Trans. Vol. LXXIII. p. 4.) when I used it for a nocturnal planet; for, day-light being sufficiently strong, there was no occasion to light the lamps. On the measuring disk were drawn concentric circles; and also a diameter, having several lines parallel to it, in one of the semicircles. If there had been time, I should have prepared a straight edge, *b e*, moveable parallel to the diameter *a d*. See Tab. XXII. fig. 2.

First measure, with the double eye-glass; power about 90. Diameter of Venus 2390. Projection 500. But the power is too low to be accurate.

Second measure; power 215. Diameter of Venus 4800. Projection 620. Here the projection is probably as much too small as the former was too large; but the planet is too low for re-

peating the measures. A mean of both may, perhaps, not be far from the truth; which gives, diameter 3595; projection 560.

Here 1797,5 being radius, and 560 sine, we find the angle *a c b*, or *d c e*, equal to 18° 9′ 8″,2.

A few very evident results may be drawn from the foregoing observations.

With regard to the rotation of Venus on an axis, it appears that we may be assured of this planet's having a diurnal motion, and though the real time of it is still subject to considerable doubts, it can hardly be so slow as 24 days. Its direction, or rather the position of the axis of Venus, is involved in still greater uncertainty.

The atmosphere of Venus is probably very considerable; which appears not only from the changes that have been observed in the faint spots on its surface, but may also be inferred from the illumination of the cusps, when this planet is near its inferior conjunction; where the enlightened ends of the horns reach far beyond a semicircle. I must here take notice, that the author we have before quoted on this subject, has the merit of being the first who has pointed out this inference, but he has overlooked the penumbra arising from the diameter of the sun; * which has certainly a considerable share in

* He mentions it upon another occasion, and says in a note, p. 313, that "*this whole penumbra, which, according to the greatest apparent diameter of Venus, extends from* 59 *to* 60′, (for what reason he fixes upon these quantities does not appear) "*measures, in the direction perpendicular to the line of the cusps, only* 0′,36." But if, according to him, the apparent diameter of the sun be 44′, (which is less than it ought to be) the penumbra must certainly extend likewise upon the surface of Venus

the effect of the extended illumination, and in his angle of 15° 19′ will amount to more than two degrees and a third. His measures are also defective; as probably the mirror of his 7-feet reflector, which was a very excellent one, was by that time considerably tarnished, and had lost much of the light necessary to shew the extent of the cusps in their full brilliancy.

I do not give the calculations I have made of the extent of the twilight of Venus, because my measures were not so satisfactory to myself as I wish them to be; nor so near the conjunction as we may hereafter obtain them; neither were they sufficiently repeated. My computations, however, when compared to those given in the paper on the atmosphere of Venus, shew sufficiently that it is of much greater extent, or refractive power, than has been computed in that paper. Those calculations indeed are so full of inaccuracies, that it would be necessary to go over them again, in order to compare them strictly with my own, for which at present there is no leisure.

I ought also to take notice here, that the same author, it seems, has taken measures of the horns of Venus by an instrument, which, in his publications, he calls a *projection table*, and describes as his own*; of which, however, those who do not know its construction may have a very perfect idea, when they read the descriptions of my lamp, disk, and periphery

over 44′ of a great circle; and, in the situation which he mentions, that is, perpendicular to the line of the cusps at the time of the greatest elongation, and when the apparent diameter of Venus is 60″, (as he makes it) it must measure 0″,384.

* See *Beiträge zu den neuesten astronomischen Entdeckungen*, p. 210. And *Selenotopographische Fragmente*, p. 63.

micrometers, joined to what I have mentioned above, of using the disk-micrometer without lamps when day-light is sufficiently strong; or even with an illumination in front, where the object is bright enough to allow of it, such as the moon, &c.

I remember drawing the picture of a cottage by it, in the year 1776, which was at three or four miles distance; and going afterwards to compare the parts of it with the building, found them very justly delineated.

I have also many times had the honour of shewing my friends the accuracy of the method of applying one eye to the telescope, and the other to the projected picture of the object in view; by desiring them to make two points, with a pin, upon a card fixed up at a convenient place, where it might be viewed in my telescope; and this being done, I took the distance of these points from the picture I saw projected, in a pair of proportional compasses, one side of which was to the other as the distance of the object, divided by the distance of the image, to the magnifying power of the telescope; and giving the compasses to my friends, they generally found that the proportional ends of them exactly fitted the points they had made on the card. All which experiments are only so many different ways of using the lamp-micrometer.

As to the mountains in Venus, I may venture to say that no eye, which is not considerably better than mine, or assisted by much better instruments, will ever get a sight of them; though, from the analogy that obtains between the only two planetary globes we can compare, (the moon and the earth) there is little doubt but that this planet also has

inequalities on its surface, which may be, for what we can say to the contrary, very considerable.

The real diameter of Venus, I should think, may be inferred with great confidence, from the measures I took with the 20-feet reflector, in the morning of the 24th of November, 1791; which, when reduced to the mean distance of the earth, give 18″,79 for the apparent diameter of this planet.

This result is rather remarkable, as it seems to prove that Venus is a little larger than the earth, instead of being a little less, as has been supposed; yet, upon the nicest scrutiny, I cannot find fault with the measures. The planet was put between the two wires of the micrometer, which were outward tangents; and they were, after each measure, shut, so as to meet with the same edge, and in the same place where the planet was measured. In this situation the proper deduction, for not being central measures, was pointed out by the index plate. The transits of the 25th were corrected for a small concavity of the wires, which being pretty thick and stubborn, were not strained sufficiently to make them quite straight, the amount of which was also ascertained by an examination of the division where the wires closed at the ends, and where they closed in the centre. The zero was, with equal precaution, referred to a point at an equal distance from the contact of the wires on each side; for they are at liberty to pass over each other, without occasioning any derangement. The *shake*, or *play*, of the screw is less than 3-tenths of a division.

The two planets, however, are so nearly of an equal size, that it would be necessary to repeat our measures of the diameter of Venus, in the most favourable circumstances, and

with micrometers adjusted to the utmost degree of precision, in order to decide with perfect confidence that she is, as appears most likely, larger than the earth.

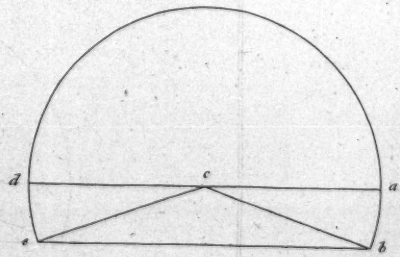
The remarkable phænomenon of the bright margin of Venus, I find, has not been noticed by the author we have referred to: on the contrary, it is said, page 310, "*this light appears strongest at the outward limb a b c, from whence it decreases gradually, and in a regular progression, towards the interior edge, or terminator.*" But the luminous border, as I have described it, in the observations of the 9th, 16th, 20th, and 22d of April, does not in the least agree with the above representation.

With regard to the cause of this appearance, I believe that I may venture to ascribe it to the atmosphere of Venus, which, like our own, is probably replete with matter that reflects and refracts light copiously in all directions. Therefore on the border, where we have an oblique view of it, there will of consequence be an increase of this luminous appearance. I suppose the bright belts, and polar regions of Jupiter, for instance, which have a greater light than the faint streaks, or yellow belts, on that planet, to be the parts where its atmosphere is most filled with clouds, while the latter are probably those regions which are free from them, and admit the sun to shine on the planet; by which means we have the reflection of the real surface, which I take to be generally less luminous.

If this conjecture be well founded, we see the reason why spots on Venus are so seldom to be perceived. For, this planet having a dense atmosphere, its real surface will com-

Fig. 1.

Fig. 2.



monly be enveloped by it, so as not to present us with any variety of appearances. This also points out the reason why the spots, when any such there are, appear generally of a darker colour than the rest of the body.

XIX. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland. *By* Thomas Barker, *Esq.; with the Rain in* Surrey *and* Hampshire, *for the Year* 1792; *and a Comparison of wet Seasons. Communicated by* Thomas White, *Esq. F. R. S.*

Read June 20, 1793.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.				Surrey.	Hampshire.	
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	Lyndon.	South Lambeth.	Selbourn.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.	Inches.	Inches.	Inches.
Jan.	Morn.	29,92	28,47	29,18	47 1/2	30	39	46 1/2	16	34 1/2	2,097	2,51	6,07	4,47
	Aftern.				49	30 1/2	39 1/2	51 1/2	25	38 1/2				
Feb.	Morn.	94	29,04	48	47 1/2	32	41	47 1/2	16 1/2	35	0,712	1, 5	1,68	1, 6
	Aftern.				49	34	42	55	26	42 1/2				
Mar.	Morn.	30,00	28,53	26	50	35	44	48 1/2	25 1/2	39	1,096	2,13	6,70	2,92
	Aftern.				51	35 1/2	45	57	30 1/2	47 1/2				
Apr.	Morn.	29,85	72	42	60	43 1/2	51	56	36 1/2	46	4,042	2, 4	4,08	2, 9
	Aftern.				62	44	53	71	39	57				
May	Morn.	91	77	49	58 1/2	45	50 1/2	58	36 1/2	47 1/2	1,660	1,49	3,00	2,51
	Aftern.				62	46	53	68	45	57				
June	Morn.	88	97	46	63	50	54 1/2	64 1/2	47	53	4,043	1,45	2,78	3,17
	Aftern.				67	53	57	77 1/2	49	62 1/2				
July	Morn.	71	29,13	41	65	53	59 1/2	66 1/2	52	57 1/2	3,674	3,98	5,16	3,81
	Aftern.				68	57 1/2	61	78	57 1/2	67 1/2				
Aug.	Morn.	83	28,89	48	69	57	62 1/2	67 1/2	50	58 1/2	2,861	2,86	4,25	2,52
	Aftern.				73	59 1/2	65	79 1/2	61	70				
Sep.	Morn.	85	57	30	61 1/2	48 1/2	55	60	41 1/2	50	3,977	2,66	5,53	3,93
	Aftern.				63 1/2	50	56	68 1/2	48	58				
Oct.	Morn.	97	72	34	58	46	49	57	35	45 1/2	1,756		5,55	4, 6
	Aftern.				59	46	50 1/2	66	46	52				
Nov.	Morn.	91	78	52	51 1/2	40 1/2	46	50 1/2	31 1/2	42 1/2	0,761		1,65	90
	Aftern.				53	39 1/2	46 1/2	56	37 1/2	47				
Dec.	Morn.	85	50	31	48	36	41	52	29	39	2,723		2,11	1,40
	Aftern.				48 1/2	36	42	54	31	41 1/2				
											29,402		48,56	32,84

THE winter was a severe one; there was a sharp frost every month from December to March, chiefly between the full and the new moons, and the intervals were often stormy and wet; but those in February, both at the middle and latter end of the month, were milder, and less wet. The beginning of March continued mild, with frequent though small rains; then followed as sharp a frost, for a week, as any in the winter. After that stormy weather into April, but warm and growing; till a violent thunder storm toward Stamford the 13th, and two days continued rain here, and in most other places, about the 18th, renewed the wet season; which lasted all summer, and was perhaps wetter in many places than here, for we had no heavy thunder storms all the summer, as they had in some parts. Whenever there was thunder this year, it was almost always cold after it, and often cold weather without it; very little sunshine, and many sharp frosty mornings both in May and June, which cut off the apples after they appeared to be set. The greatest rains this summer were after the middle of April; before the middle of May; about the 8th of June; the 21st of July; the 18th of August; and 14th of September: those in April, June, and July, made floods, the two latter of which did great damage to the meadow hay; and there were frequent, sometimes almost daily, lesser rains. The intervals of fair and fine weather were short, and not many, and those not always warm; the beginning of May, and about the 21st; the beginning and end of June; the beginning of July; and, what was the finest time this summer, the first half of August. During this, in general so very wet a season, the hay and harvest were got in, and, where they were not flooded, I think with less damage than might have

been expected. The latter hay was got up during the fine time in August; some of the harvest in a tolerable time the beginning of September; and what was delayed by the almost daily rains for two-thirds of September, was finished in a fine time the beginning of October; the crop of wheat was tolerable well, but barley, oats, and peas, were dear.

This year was the wettest since 1782, which, with 1774, and some others, exceeded it; and this, like those two years, began to grow less wet the beginning of October. Yet the frequent rains after that, though less in quantity, kept the ground from drying, which was already too wet, and the roads continued uncommonly torn up all winter; and December, being wetter, increased it. The last six weeks of the year were in general dark and cloudy, or misty; very little sun, and not much frost, and so far seems to promise an open winter; but December was a stormy time; several great ones, and some great rains and floods.

A Comparison of wet Seasons.

Twelve Months.

No. I.

	1774.		1782.		1792.	
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January	3,308		2,333		2,097	
February	1,946	5,254	0,636	2,969	0,712	2,809
March	2,728	7,982	1,923	4,892	1,096	3,905
April	1,523	9,505	6,125	11,017	4,042	7,947
May	3,142	12,647	5,722	16,739	1,660	9,607
June	2,483	15,130	1,295	18,034	4,043	13,650
July	3,227	18,357	2,697	20,731	3,674	17,324
August	3,910	22,267	3,114	23,845	2,861	20,185
Septem.	8,000	30,267	5,151	28,996	3,977	24,162
October	1,156	31,423	1,502	30,498	1,756	25,918
Novem.	1,530	32,953	1,074	31,572	0,761	26,679
Decem.	2,282	35,235	0,517	32,089	2,723	29,402
	1773	29,376				
	1775	31,699				
Three years		96,310				

No. II.

Oct. 3, 1773 to Oct. 2, 1774.		Dec. 1774 to Nov. 1775.		Oct. 1791 to Sept. 1792.		Feb. 1763 to Jan. 1764.		1768.	
	Inches.		Inches.		Inches.		Inches.		Inches.
Oct. 3	2,615	Dec.	2,282	Oct.	3,319	Feb.	2,882	Jan.	2,834
Nov.	3,605	Jan.	1,973	Nov.	4,231	Mar.	0,919	Feb.	3,062
Dec.	2,897	Feb.	2,522	Dec.	1,150	April	0,692	March	0,391
Jan.	3,308	Mar.	1,728	Jan.	2,097	May	2,304	April	2,023
Feb.	1,946	April	1,035	Feb.	0,712	June	2,420	May	1,622
March	2,728	May	0,900	March	1,096	July	5,657	June	4,521
April	1,523	June	0,887	April	4,042	Aug.	2,929	July	2,402
May	3,142	July	4,078	May	1,660	Sept.	3,307	Aug.	1,720
June	2,483	Aug.	4,760	June	4,043	Oct.	1,606	Sept.	3,025
July	3,227	Sep.	5,670	July	3,674	Nov.	1,894	Oct.	3,119
August	3,910	Oct.	3,480	Aug.	2,861	Dec.	3,525	Nov.	4,040
Septem.	8,000	Nov.	3,570	Sept.	3,977	Jan.	3,984	Dec.	2,146
Oct. 1 & 2	0,340								
	39,724		32,885		32,862		32,125		30,905

No. III.

Three years.

	May 9, 1773, to May 8, 1776.				17 months.—May 9, 1773, to Oct. 8, 1774.	
	1773.	1774.	1775.	1776.	1773.	1774.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
Jan.		3,308	1,973	2,511		3,308
Feb.		1,946	2,522	3,195		1,946
March		2,728	1,728	1,518		2,728
April		1,523	1,035	0,887		1,523
May	6,770	3,142	0,900	0,860	6,770	3,142
June	2,389	2,483	0,887		2,389	2,483
July	1,077	3,227	4,078		1,077	3,227
August	3,379	3,910	4,760		3,379	3,910
Septem.	2,812	8,000	5,670		2,812	8,000
October	2,621	1,156	3,480		2,621	0,460
Novem.	3,605	1,530	3,570		3,605	
Decem.	2,897	2,282	1,096		2,897	
	25,550	35,235	31,699	8,971	25,550	30,727
	Three years time 101,455					56,277

No. IV.

Nine months.

Jan. 6, to Oc. 6, 1774.		Jul. 1775 to Mar. 1776.		Jan. 1782 to Sept.		May, 1773 to Jan. 1774.		May. 1763 to Jan. 1764.		April, 1768 to Dec.		Jan. 1792 to Sept.	
	Inches.		Inches.		Inches.		Inches.		Inches.		Inches.		Inches.
Jan.	3,308	Jul.	4,078	Jan.	2,333	May	6,843	May	2,304	Apr.	2,023	Jan.	2,097
Feb.	1,946	Aug	4,760	Feb.	0,636	June	2,389	June	2,426	May	1,622	Feb.	0,712
Mar.	2,728	Sep.	5,670	Mar.	1,923	July	1,077	July	5,657	June	4,521	Mar.	1,096
Apr.	1,523	Oct.	3,480	Apr.	6,125	Aug	3,379	Aug	2,929	July	2,402	Apr.	4,042
May	3,142	Nov.	3,570	May	5,722	Sep.	2,812	Sep.	3,307	Aug	1,720	May	1,660
June	2,483	Dec.	1,096	June	1,295	Oct.	2,621	Oct.	1,606	Sep.	3,025	June	4,043
July	3,227	Jan.	2,511	July	2,697	Nov.	3,605	Nov.	1,894	Oct.	3.119	July	3,674
Au.	3,910	Feb.	3,195	Aug	3,114	Dec.	2,897	Dec.	3,525	Nov.	4,040	Aug	2,861
Sep.	8,000	Mar.	1,518	Sep.	5,151	Jan.	3,308	Jan.	3,984	Dec.	2,146	Sep.	3,977
Oct.	0,460												
	30,727		29,878		28,996		28,931		27,632		24,618		24,162

No. V.

Six months.

April 12, 1782, to Oct. 11.		July to Dec. 1775.		April to Sept. 1774.		April to Sept. 1792.	
	Inches.		Inches.		Inches.		Inches.
April 12	5,375	July	4,078	April	1,523	April	4,042
May	5,722	Aug.	4,760	May	3,142	May	1,660
June	1,295	Sept.	5,670	June	2,483	June	4,043
July	2,697	Oct.	3,480	July	3,227	July	3,674
August	3,114	Nov.	3,570	Aug.	3,910	Aug.	2,861
Septem.	5,151	Dec.	1,096	Sept.	8,000	Sept.	3,977
Oct. to 11	0,950						
	24,304		22,654		22,285		20,257

No. VI.

Three months.

July 3, 1774, to Oct. 2.		1775.		1782.		1770.	
	Inches.		Inches.		Inches.		Inches.
July 3	3,227	July	4,078	Mar.	1,923	Oct.	3,114
August	3,910	Aug.	4,760	Apr.	6,125	Nov.	7,818
Septemb.	8,000	Sept.	5,670	May	5,722	Dec.	2,613
Oct. 1 & 2	0,340						
	15,477		14,508		13,770		13,545

1763.		1737.		1784.		May 25, 1792, to Aug. 24.	
	Inches.		Inches.		Inches.		Inches.
July	5,657	Aug.	6,300	May	2,890	May 25	0,770
Aug.	2,929	Sept.	3,465	June	3,810	June	4,043
Sept.	3,307	Oct.	2,025	July	5,080	July	3,674
						Au. to 24	2,511
	11,893		11,790		11,780		10,998

No. VII.

One month.

	Inches.
1774 September 3	7,930
to Oct. 2	0,340
	8,270
1770 November 6	7,818
to Dec. 5	0,410
	8,228
1773 May - -	6,843
1736 July -	6,550
1737 August -	6,300
1782 April -	6,125
1757 August -	6,057
1782 May -	5,722
1775 September	5,670
1763 July - -	5,657
1743 July - -	5,230
1776 August -	5,200
1782 September	5,151
1792 August 16	2,762
to Sept. 15	2,346
	5,198

No. VIII.

Abstract.

	Inches.
Three years time - -	101,455
Three calendar years, 1773, 1774, and 1775 - -	96,310
Seventeen months - -	56,277
Twelve months - -	39,724
Nine months - -	30,727
Six months - -	24,304
Three months - - -	15,477
One month - - -	8,270

The year 1792 was a very wet one, and by many imagined to exceed all others, but that does not appear to be fact; the wet of last year is fresh in memory, that of former years is more forgotten. It might seem the wetter, because the autumn of 1791 was wet, so that there was a long continuance of it; and perhaps there might be more rain in some other places than here, as we had no great thunder storms all the summer at this place, which they had in several parts, some not many miles off. The wettest years

here were about 1774 and 1782, which I have therefore compared with last year, in No. I. where I have set down the whole rain, and cast up the sum, from January the first, to the end of every month, in each year: and it appears, that to the end of January, to the end of February, and of March, the wettest was 1774, the next 1782, and 1792 was less wet than either of them. The very wet April and May in 1782 altered the order of them; and to the end of April, of May, of June, of July, and of August, the wettest was 1782; the next 1774; and the last 1792. September, 1774, that wettest of all months in fifty-seven years, altered the order again to 1774, 1782, 1792; and it continued so to the end of the year. In No. II. I have given some of the greatest twelve months, whether beginning with January or not; and the greatest 365 days is from October 3, 1773 to October 2, 1774, which is 39,724 inches; and all that I have here given exceed 1792. In No. III. is the greatest three years, from May 9, 1773 to May 8, 1776, which is 101,455 inches; and the greatest seventeen months, from May 9, 1773 to October 8, 1774, is 56,277 inches. In No. IV. are the greatest nine months, January 6 to October 6, 1774, 30,727 inches; and several others, to 1792, 24,162 inches. In No. V. are several of the greatest six months, from 1774, 24,304 inches, to 1792, 20,257 inches. In No. VI. are several of the greatest three months, from 15,477 inches in 1774, to 10,998 inches in 1792. The greatest month last year was, from August 16 to September 15, 5,108 inches, but I have had thirteen greater; the most of all was in 1774, 8,270 inches; the rest are set down in order in No. VII.; and the last is that in 1792. Lastly, in No. VIII. I have set down together the wettest times in all the several cases.

At Selbourn, between Alton and Petersfield, in Hampshire, which lies at the NE foot of a steep hill, that rises an hundred yards perpendicular above it, they have half as much more rain as I have; there was 48½ inches last year, as it is set down in the first page; but they had 50¼ inches in 1782, which is something more. But I was surprised to see, in the Supplement to the Gentleman's Magazine, page 1197, that Mr. GOUGH says there was 83½ inches of rain at Kendal last year. This is an astonishing quantity; though it is a hilly country, it is almost four times my common year, and above double the greatest; and I should have thought it enough, in latitude 54°, to have made the whole country a marsh.

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	J. Dickson Fasciculus secundus Plantarum Cryptogamicarum Britanniæ. Londini, 1790. 4°	Mr. James Dickson.
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	Observations on Construction, by J. Glenie. 8°	
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